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A Realist Approach to Equilibrium Analysis — **Jörg Guido Hülsmann**

Are MMMFs Money? — **Jeff Haymond**

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A REALIST APPROACH TO EQUILIBRIUM ANALYSIS

JÖRG GUIDO HÜLSMANN

Economic science, as handed down to us from Menger and Mises, explains observed human behavior by referring to other features of the real world. Both the phenomenon to be explained and the explanation itself are thus strictly realistic—a charm and advantage of the Menger-Mises approach as compared to other approaches.

However, there are exceptions. The most blatant is the way Mises (1998, pp. 248ff.) conceived of the nature of equilibrium analysis. His account relies on an intellectual fiction, namely, on what he has baptized the evenly rotating economy (ERE). The ERE is admittedly unrealistic; it is an "imaginary construct" that has no—and can never have any—counterpart in the real world.

The present article offers an entirely realistic account of equilibrium analysis, thus closing a disturbing gap in economic science. We will argue that human choice involves a dichotomy of success and failure, and that equilibrium analysis is the method of explaining observed success by contrasting it to counterfactual failure, and observed failure by contrasting it to counterfactual success. This approach gives us the clue needed to reconstruct the role of equilibrium analysis within economic science and economic policy, and to discern the problems of applying it.

THE ESSENCE OF EQUILIBRIUM ANALYSIS

On a purely physical level, choosing means to select between competing projects, that is, between mutually exclusive ways of using our brains, bodies, and other objects that we control. We cannot pursue all of our goals at the same

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time. We have to decide between the realization of some *and* the postponement of other projects. Whatever we choose to realize through our action is bought at the expense of something else that, because of that very choice, cannot come into existence.

However, there is also a value-aspect of choice, and this aspect confronts the acting person with a problem. The fundamental fact is that the various alternatives do not have the same relative importance or value. Some are more important than others, even though this relative importance varies from one person to another and depends on the particular circumstances of time and place in which one and the same person makes his decisions. The acting person therefore has to identify to the best of his abilities the alternative that, under the given circumstances, is for him the *most important* one.

This identification process is future-oriented and, therefore, heavily speculative. For example, when buying stocks, one estimates the future prices of alternatives. Yet, even when buying a shirt, one not only has to determine which shirt is best now, but how one will value it in the future.

Now, subjectively – that is, as far as the mere opinion of the acting person is concerned – he always chooses the most important project. By the very fact that he acts in this manner rather than another, he "demonstrates" his belief that this action is better than any other he could have performed instead. *Ex ante*, then, his choice is always optimal.[1]

However, his subjective deliberation does not necessarily grasp what is *really* the most important thing for him to do under the circumstances. This applies both to the ends he chooses to attain and the means he chooses to realize them. The plain fact is that the beliefs guiding his choice of ends and means might be right – but they might just as well be wrong.

Often we find, *ex post*, that no other behavior would have been preferable to our chosen behavior. We then think we made the right choice, and call our action successful. Economists describe this phenomenon using more technical jargon. They say that the successful actions of entrepreneurs are based on "rational expectations," or that they stem from "perfect foresight." When *all* entrepreneurs act successfully, economists say they are "coordinated," or that the market is in "general equilibrium." All these expressions are synonymous in the sense that they refer to successful human action, as distinguished from action that is less, or is not successful.

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[1]The expression "demonstrated preference" is Rothbard's (1956). However, see also Windelband (1904, pp. 35ff.), Schumpeter (1908, pp. 64ff.), and Mises (1998, pp. 95, 102). In addition, see the analogous argument of Herbert Spencer (1970, pp. 75ff., esp. p. 79) for limiting the scope of political philosophy to the study of justice.

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However, we find at least as often, *ex post*, that we should have done something other than what we actually did. Other actions would have been preferable. We then think that we made the wrong choice, and say that we erred.

This experience is, of course, familiar to all human beings. There is something like "right choice" (correct judgment, success), as opposed to "wrong choice" (error, failure), and it is meaningful to distinguish between them, for our subjective beliefs about the world do not always reflect the world as it really is.

This distinction is also reflected in the vocabulary of price theory. Economists have traditionally distinguished "market prices," established as a consequence of *ex ante* deliberations, from "right" prices (or natural prices, equilibrium prices, etc.). The former does not necessarily coincide with the latter. Market prices can be equilibrium prices, yet can also be – and are indeed most likely – disequilibrium prices, because of the ubiquity of error.

Error has many psychological faces that are difficult to grasp in exact terms. We call them whims, fancies, follies, greed, jealousy, illusion, etc. However, the economic aspect of error can be precisely circumscribed. Error is constituted by the fact that a person chooses to pursue a project that is less important for him than another project he could have pursued, but did not because of that very choice. In short, error is the failure of the choosing person to select the project most important for him. This point is not only common sense, but, as we have just seen, is rooted in praxeological bedrock – namely, in the fact that both success and failure are contained as possibilities in human choice. Not surprisingly, therefore, the distinction between success and failure is familiar to all equilibrium theories in economic science. And most of them even rely on the assumption that, under any circumstances, there is a best option in comparison to which all others are worse. As Frank A. Fetter said, "in any given set of conditions there is a best proportion in which to combine agents."[2]

Now, the crucial fact that needs to be emphasized is that our distinction is dichotomous. All human actions are either successes or failures. Either we could have performed a more important action, or no better alternative was available. Hence, any possible choice is either right or wrong, any possible action either a success or a failure.

[2]Fetter (1915, p. 130). Similarly, Hicks (1946, p. 255, n. 1) stated, about the assumption that the system of relative prices is uniquely determined: "If it is not justified anything may happen." See also Menger (1883, app. 6), Knight (1956, p. 164), Hicks (1965, pp. 24, 41), Nash (1950, 1951), Hahn (1973, p. 7), and Harsanyi and Selten (1988). For the view that there are several or multiple equilibria, see, for example, Hildenbrand and Kirman (1988), Billot (1995), and Creedy and Martin (1994). We will deal with this latter view below.

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A casual reflection shows that everybody, in his daily life, makes ample use of this common-sense dichotomy of success and failure. Thus, we say things like, "I should have read Mises rather than Marx at the age of twenty-three," or, "he should have become a lawyer rather than a painter," or, "how good that we decided to go to this concert tonight." All such statements judge a real-world action in terms of an explicit or implicit reference to unrealized alternatives conceived to be either inferior or superior to the alternative that came to be realized. And, irrespective of whether the action under scrutiny is a success or a failure, we must necessarily apply *the same method* to describe what it is—in both cases, we must judge the actual choice by reference to its counterfactual alternatives.

The foregoing everyday statements would not by themselves justify any closer examination of their logical structure. Yet, here they are relevant because that structure is identical to that which we encounter in quite sophisticated descriptions of factor pricing and income distribution on the market. In fact, as we shall see, *equilibrium analysis is nothing but the method we just outlined.* It describes what exists in the real world as being more or less important than what could have existed instead—from the standpoint of the acting persons involved. The difference between the application of equilibrium analysis in daily life and its application in economic science is merely a gradual one. The latter is more sophisticated, since it generally focuses on more remote implications of the fact that an observed state of affairs is either more or less important than the one that could have existed.

Consider, therefore, the fundamental theorem of the theory of production and distribution. It states that, in general equilibrium, the price of a factor of production (that is, the marginal income of its owner) is identical to that factor's discounted marginal value productivity (DMVP). All elements of this description refer to features of a particular state of affairs—one without entrepreneurial error. *If* no entrepreneur errs, the market is said to be in equilibrium. Furthermore, *if* the market is in equilibrium, the prices paid for factors of production, whatever they may be, are called the DMVPs of these factors. By contrast, if at least one entrepreneur commits an error, market prices will *ipso facto* differ by some amount from the DMVPs. These differences are called profit (if the marginal income is higher than it would have been in equilibrium) and loss (if it is lower).

Fundamentally, error on the market can occur here in either one of two directions. Either the factor under consideration—say, a truck—is paid less, or it is paid more than its DMVP. If it were paid less, there would have been an opportunity for arbitrage. Another entrepreneur could have bid this factor away and still profited from it. The very fact that this did not happen demonstrates that

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an error occurred on the part of *other* entrepreneurs. As a consequence, our entrepreneur realizes a special kind of income, namely, profit. By contrast, if our entrepreneur paid more than the DMVP for the truck, his income would be negatively affected. It would be lower than it would have been had he paid a lower price. In this case, the error occurs on *his* part rather than on that of other entrepreneurs.

Let us emphasize that, just as in our examples from daily life, the above analysis does not touch on the question of whether the market actually is, or is not, in equilibrium. In fact, this question does not need to be answered to do what we have done: describe what market participants do as being, from their standpoints, more or less important than what they could have done.

We are now in a position to define equilibrium analysis. *Equilibrium analysis is the method of comparing actual behavior with its counterfactual alternatives in terms of success and failure.*

Applying this method, we can describe a common aspect in all imaginable cases of market behavior. There are indeed only three possibilities: (1) a factor is paid according to its DMVP, (2) it is bought for more, or (3) it is bought for less than its DMVP. That is, either there is no error at all, or an error occurs in one of the two directions of "too much" or "not enough."

Let us point out that this is not to say that the role of DMVP in equilibrium analyses of the market is to be a standard by which we define profit and loss. For it would be equally pertinent to say that the occurrence of profit and loss is the standard by which we define DMVP. The point is that equilibrium analysis does not give us a *picture* of "normal" reality; rather, it is a *method* to describe reality, and this method can be applied irrespective of what observed reality happens to be. Observed equilibrium can be meaningfully conceived of only by reference to errors that could be avoided, just as observed error can be understood only by reference to a foregone equilibrium. This putting-in-relation of what is and what could have been—in terms of success and failure—is what equilibrium analysis does.[3]

[3]The counterfactual nature of equilibrium analysis is the reason why equilibrium analysis, although it explains observed facts by relating them to other facts, is not empirical in the sense that all elements of the analysis (the behavior to be explained *and* the fact that explains it) can be observed. Since the explaining fact in equilibrium analysis is a foregone alternative, it cannot be observed. Our very knowledge of the existence of foregone alternatives is not derived from observations, but from acquaintance with the *a priori* nature of human action. All we can see, for example, is that a shop opens its doors one day, that people go in and out carrying commodities in one direction or the other, and that on another day the shop closes its doors forever and something else takes its place. These observations do not reveal whether the shop owner was forced to cease operations against his initial intentions (bankruptcy), whether he was forced to cease operations in accordance with his

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A standard criticism of equilibrium analysis holds that it does not adequately reflect observed behavior. What such critics have in mind is that human action is often, is perhaps even usually, not in equilibrium—a pertinent observation which, however, misses the main point. It is true that human life is fraught with errors. Each time we choose to pursue an action other than the most important one we commit an error in the sense of economic theory. Yet this does not refute equilibrium analyses—quite to the contrary! We can analyze all instances of human error in the real world only because choice implies the possibility of success *and* failure. We could not even conceive of something like error without having in mind an alternative action compared to which an observed action could be erroneous. We could not identify a single instance of error in the real world if we did not presuppose the existence of a foregone success.

Hence, the applicability of equilibrium analysis does not at all rely on the question of whether the world actually is or is not in equilibrium. And whoever seeks to point out success or failure in any given situation inevitably applies this method because otherwise he could not grasp the success–failure aspect of life.

In conclusion, let us stress that equilibrium analysis is entirely realistic since all its constituent factual and counterfactual elements can be found in real human action. It does not postulate that human beings "normally" or "generally" do not commit errors. And while it is a method *for* the exact description of the observed real world, it is not itself such a description.

SOME PROBLEMS CONSIDERED

The Problem of Indifference

A possible objection to our approach could stress that human beings occasionally are indifferent as to their options, or that their decision making is sometimes "fuzzy" (Billot 1995). In this case there would be no genuine dichotomy between the best alternative, which we called success, and other alternatives, which are relative failures. And it would not be clear at all which meaning should be attached to the notion that alternatives differ in their objective importance.

Before we take a closer look at this contention, let us point out that it would also apply, in at least some sense, to all other approaches to equilibrium analysis. For even though the notion of indifference is an important element in

initial intentions (which might be a case of fraud), or whether he just retired. Our knowledge of what bankruptcy is, or fraud, or retirement does not stem from observations at all, but from our knowledge of such invisible features of human action as choice and intentions.

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contemporary mainstream analysis of value and choice, it is no substitute for choice. The main purpose of stressing indifference is to provide the groundwork for a quantitative treatment of value. It does not replace the role of choice in the argument of equilibrium theorists. The fundamental fact is that one cannot make sense of equilibrium other than by reference to somebody correctly choosing one option rather than another. Hence, even if valid, the above contention would only lead to a limitation and reformulation of our results. Rather than saying that, "in equilibrium analysis that we contrast observed success with a foregone failure," we would have to say "we contrast a chosen action to a set of foregone actions (which might be equally desirable)." And we would have to admit that explaining success by referring to avoided failure, as well as failure by referring to foregone success, is only possible in the case of actions involving choices between superior and inferior alternatives.

This limitation and reformulation is superfluous however: it is futile to stress indifference as an objection to our approach to equilibrium analysis. The fundamental fact is that indifference is a *psychological* phenomenon. What economists have in mind when they refer to indifference, and what people mean when they acknowledge its existence, is a state of mind in which an individual contemplates two alternatives, but does not or cannot pick one because he finds them equally desirable. However, our argument does not rely on considerations about states of the human psyche, but on an analysis of human *action*. Whatever else might be obscure about action, there can be no doubt that if a given individual does something at a given moment, and what he does can always be distinguished from alternative things that he does not do. This is not to deny that a given behavior has various aspects, or that it could serve different purposes—for example, by taking a walk in the park, I can enjoy the landscape, think about indifference, and relax. What it means is that, as far as human action is concerned, all alternatives do not have the same status. Rather, one alternative is realized, while all others are not.

Genuine choice alternatives, which come to bear on action and not merely on the mind, are therefore categorically unequal. And genuine action—not the musing of the brain about action—is never indifferent. The very activity of feeling indifferent about two alternatives is itself an expression of preference: one *prefers* to indulge in indifferent feelings, thus foregoing alternative activities. It is therefore conceivable that an individual is in his mind indifferent about the alternatives he faces. However, it is impossible that he is indifferent about them in his actions. He always does something. And by virtue of the fact that he does A rather than B, he demonstrates that he is not indifferent as to these particular actions, but prefers the former to the latter.

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The Problem of Multiple General Equilibria

Another aspect of the problem we just discussed concerns the possibility of multiple general equilibria. Consider the case of a husband and his wife watching television. The value scale of the husband is as follows:

(1) watch a football game with his wife
(2) watch a romantic movie with his wife
(3) watch the football game without his wife
(4) watch the romantic movie without his wife

The wife has slightly different preferences:

(1) watch the romantic movie with her husband
(2) watch the football game with her husband
(3) watch the romantic movie without her husband
(4) watch the football game without her husband

This scenario is characterized by two features that seem to contradict what we said about equilibrium analysis.

First, it is impossible that both husband and wife end up with what they most want—that is, that they reach *general* equilibrium. It thus follows that general equilibrium is not possible under all circumstances, for there are conditions that allow only for the contest of second- and third-best outcomes.

Second, there are several outcomes that seem to be ranked equally preferable. When the couple chooses to watch the football game, the husband realizes his first preference, his wife her second. When they watch the romantic movie, the wife realizes her first preference, her husband his second. Since there is no rationale to accord a higher importance to the wife's preferences than to the husband's—or vice versa—one could argue that both of these outcomes are equally desirable. We thus have a case of multiple general equilibria.

This argument is fallacious, however, for the same reason the reference to indifference is fallacious. The fundamental error is to conceive of value scales as purely psychic entities that can be analyzed independent of human action. Yet there is no such thing as value or a value scale detached from concrete action. And as far as concrete action is concerned, it is obvious that there never can be something like "multiple action," but only one action at any point in time. This action can be a relative success or a relative failure, as counterfactual equilibrium analysis will reveal, but, irrespective of whether it succeeds or fails, the only meaningful procedure is to first look at what people do, and then compare it to what they might have done.

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Assume, therefore, that the couple watches the football game together. There is, then, no problem with explaining the husband's behavior in equilibrium terms: this common activity is his most preferred outcome. But what can we say about the wife? Is her activity not a relative failure since she would have enjoyed watching the romantic movie with her husband even more? Again, from a psychological point of view, this might be so, but this perspective does not concern us here. We are interested only in whether she has performed the best possible action under the given circumstances. Her husband watches football. Given this fact, the best possible action for her is to join him rather than watch the romantic movie in another room, on another television, alone.

But what if the couple had watched the romantic movie together? Would we not have to admit, for exactly analogous reasons, that this too is an equilibrium outcome, thus acknowledging willy-nilly the case for multiple equilibria? With this question, we are again in the realm of psychological speculation, and the only way to avoid this pitfall is, as we have said, to first look at what people actually do, and then compare that to the other alternatives. The assumed fact was that the couple *did not* watch the romantic movie. They watched the football game. If we accept this fact, we can explain their behavior with the help of equilibrium analysis. If we do not accept it, we throw ourselves into the realm of fancies, desires, and fantasies—the realm of poetry, not of science.

In light of these considerations, it also becomes obvious that all I-know-that-you-know-that-I-know-etc. paradoxes are not paradoxes of action (and therefore paradoxes for economic analysis), but paradoxes of psychological deliberation.[4]

It is true, for instance, that in social games like chess or tennis, the behavior of each player determines the success of both. Player A wants to beat player B, who wants to beat player A. The outcome of their game depends on the degree to which both anticipate the actions of the other. Now, a plan in which A seeks to account for the behavior of B can never be successful on *a priori* grounds since it is possible that B anticipates A's plan in his own plan. Yet, neither can B's plan be successful on *a priori* grounds, since A could engineer a still better plan that takes into account B's plan, and so on.

None of this has any effect on the fact that both A and B *do* something, and that, as a consequence of these actions, one of them will win and one will lose. No paradox can possibly appear that will impact this fact of action. And it is in

[4]It is no accident that one of the pioneers of game theory unearthed these problems with the famous "Holmes-Moriarty paradox." See Oskar Morgenstern (1928, p. 98).

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light of this fact that we can again explain the behavior of both players in terms of equilibrium analysis.

Yet, do these games, which by their very design have winners and losers, not exclude the possibility of *general* equilibrium? As we shall see, this is not the case.

First, participation in the game is voluntary – otherwise, it would not be a game. And since participation is voluntary, one has always the choice between taking part in the game and not taking part in it. Persons who consider playing for the sake of winning (prize-seeking players) can therefore avoid games they are going to lose, and can do so under all conceivable circumstances as long as participation is voluntary. (There can be no general equilibrium involving only prize-seeking players, of course, for at least one of them would see his endeavors thwarted.)

Second, in the case of what one might call true players, a similar result obtains. The true player plays for the mere sake of playing. He wants to win not because he prefers winning to losing, but because this is how the game is played. In fact, he prefers "playing *and* losing" to "not playing at all" – that is, to all other activities. General equilibrium is therefore possible in this case as well.

The Problem of Objective Value

Comparing success and failure presupposes some criterion by which one can gauge whether a given action is "really" more or less important than an alternative action that could have taken its place. Yet, how is it possible, one might ask, to ascertain the *objective* importance of different projects? Although it seems a given that human beings choose – that is, exercise their subjective judgment – it is not at all clear how it is possible to distinguish alternatives that are objectively more important from those that are objectively less so.

There can be no doubt that this is a considerable problem. However, as we shall see in more detail below, this problem relates to the *application* of equilibrium analysis rather than to its conceptual framework as such. For our present purposes, it is entirely sufficient to make the general point that different alternatives *do* have different objective values, even if we cannot tell exactly what they are.

What we mean by saying that a choice alternative has an "objective" value is that its value does not depend on the choice itself, but on something else entirely. This something else could still be bound up with the choosing person – we only stipulate that it is different from the choice itself. For if the value of an alternative were to depend on choice itself, then everything we choose or

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do would demonstrate not only that we *think* this action the most valuable one, but that it truly *is* the most valuable one. Each individual would then be in a perpetual state of equilibrium, and the only question left would relate to the scope and existence of general equilibrium.

However, the idea that each individual determines by his own choices what to him is more or less valuable is self-contradictory. He who chooses must from the outset presuppose that the projects he can realize can be ranked according to their objective importance. If he did not presuppose that one state of affairs were more objectively important than another, he could not choose at all. For if all projects were equally important to him, his choice would make no difference.

Moreover, the very notion of subjectivity is meaningful only if one presupposes an objective reality to which subjective value judgments refer. Something does not have high utility *because* we value it highly. Our valuing must be reasonable; it must assess the object's utility correctly. In other words, something is not (objectively) important *because* we think it is or want it to be. Our thoughts must be fitting and our wants rightly understood to gauge its real importance. Fasting twenty days per month, for example, will have certain effects on my body and character. If the bundle of effects that results from such fasting is really more important than the bundle of effects all other actions would produce, then fasting twenty days per month is the proper behavior. I then *rightly* prefer fasting, and if I preferred other actions we would have to qualify them as fancies that prevent more important action.

Man *can* succeed in choosing the most proper, most important action, and through his creativity can enlarge the realm of possibilities. However, it is not *because* he thinks an action the most important that this it is. Similarly, it is not *because* he considers an action to be just that it *is* just. Success on the economic level and justice on the moral do not depend on our intentions and wishful thinking, but on whether or not our action is objectively proper.

In light of these considerations, one understands that, in a way, it is wrong to represent economics as a subjectivist science. It is impossible to conceive of choice and value judgments without assuming the existence of an objective importance of options, even if this objective importance is valid only for one individual at one point in time, and even if it is difficult to discern objective importance in applied analysis.[5] A scientific treatment of objective value is

[5]The distinction between the subjective and objective importance of choice alternatives has a prominent if neglected place in the history of subjective value theory. Thus, Condillac (1795, p. 6) and Jevons (1965, p. 38) called the objective importance of a project its utility, distinguishing it from its subjective importance, which they called its value.

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possible because this concept refers to an aspect of individual choice. However, as an emanation of choice, objective value is necessarily relative. There is no such thing as absolute value; whether subjective or objective. And both subjective and objective values are always bound up with individual human action.

Successful Action and Perfect Foresight Compared

It might be useful to extend the foregoing discussion by clarifying the relationship of successful action and perfect foresight, which has a long tradition in our science.[6] Many economists believe that, for equilibrium economics to be empirically meaningful, it is necessary to assume that market participants have "perfect foresight," "perfect knowledge," "perfect information," etc.

The fact that successful action in some sense implies perfect foresight should not convince us that we can overlook differences between the present approach and those of our predecessors. The great advantage of referring to successful action is that such reference does not imply the absence of uncertainty. Economists commonly believe that they have to offer reasons *why* human beings should have perfect foresight. Some argue that equilibrium refers not to reality, but to an imaginary state of affairs in which conditions no longer change, which therefore would bring about certainty. Others argue that market participants have rational expectations, etc. The offshoot of all such theories is that the possibility of error is banished from equilibrium economics. In equilibrium, man "knows" what will happen in the future.

This point of view has received a well-deserved and devastating criticism by G.L.S. Shackle (1972). If equilibrium deals with a world without uncertainty, he argued, it does not apply to our world.

By contrast, we have seen that it is realistic to refer to successful action in counterfactual equilibrium analysis. This analysis does not presuppose an unchanging universe or certainty, but can be applied to any instance of concrete human action simply because human action *can* be successful under uncertainty. Moreover, as we have argued, it is immaterial for economic science whether real action *is* successful because the notion of success is only a comparative element of our understanding of the world.

Let us pursue this argument further. The success of one's action depends crucially on a pertinent *judgment* of future data with which that intended action will be confronted. These data can never be *known* in the sense that they can be experienced. Any equilibrium construct that presupposes that human

[6]See, for example, Knight (1985, p. 76f.), Hayek (1928, p. 39), and Hicks (1946, pp. 6, 123ff.).

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beings know all data is therefore unrealistic. However, economists do not have to make assumptions about what market participants know or expect, or about how they come to have the knowledge or expectations they happen to have.[7] The data, or given conditions of human action, are not given in the sense that they are known but in the sense that they exist independently of human action. Neither does "given" imply that all conditions are given *now*. Some conditions will be given in the future only, yet are and will be given in the sense that they are independent of our recognition and choices. Acting man can hardly know everything. He can, though, act successfully under all conceivable conditions since this means nothing but that, in any situation, he can choose the most important action.

One might object that it is contradictory to insist on freedom of choice while cherishing the notion that a market participant can correctly anticipate another's decisions. How can he do this? If others are truly free there should be no way for him to determine how they will choose.

However, determining and anticipating choice are two different things. In order to *determine* some future event, we have to know all the factors bringing it about. This is clearly not the case as far as human action is concerned. Yet we can *anticipate* human action without knowing any of these factors. We can even anticipate it by chance, for instance—and for this reason alone, successful action or equilibrium is possible under uncertainty.

It follows, then, that one needs to revise equilibrium economics, at least in some respects. However, it is our conviction that introducing the category of successful action does not necessitate any major adjustments of established doctrine. In particular, it does not necessitate a modification of the theory of profit and loss since this theory, at least in Mises's account, does not stress uncertainty (as distinguished from risk) and does not even raise the question of whether risk and uncertainty are subjective or objective notions. Instead, Mises casts his exposition of the nature of profit and loss exclusively in terms of entrepreneurial judgment and choice:

> What makes profit emerge is the fact that the entrepreneur who judges the future prices of the products more correctly than other people do buys some or all of the factors of production at prices which, seen from the point of view of the future state of the market, are too low.... On the other hand, the entrepreneur who misjudges the future prices of the products allows for the factors of production prices which, seen from

[7] In particular, economics is not based on a theory of the acquisition and communication of knowledge. See on this point Selgin (1990, p. 62f.), Hoppe (1996), and Hülsmann (1997).

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> the point of view of the future state of the market, are too high. . . . Thus profit and loss are generated by success or failure in adjusting the course of production activities to the most urgent demand of the consumers.[8]

It is true that, for Mises and for many other economists, failure is a consequence of changing conditions. However, the point is that this issue can be treated separately. Whether success and failure have "causes," what the specific causes of success and failure are, and whether these causes are intelligible is one question. What profit and loss are is another. In respect to this latter question, we can state that our approach is perfectly congruent with established doctrine. And as far as the *causes* of success and failure are concerned, even if they were to exist, there is no need to identify them in equilibrium analysis, for it covers both equilibrium and profit and loss without regard to their possible causes.

The Meaning of Equilibration and Arbitrage

Since human action is either successful or in error, *there is no middle ground between equilibrium and disequilibrium.* Based on this observation, we can make two further inferences that have some relevance to the current debate.

First, one has to reject the fashionable notion of "equilibration," which implies a movement in time from a situation of disequilibrium to one of equilibrium. The fallacy of this notion lies in the attempt to understand equilibrium as a feature of the environment of action, rather than of action itself. As advocates of equilibration see it, human action transforms *its environment (or circumstances or conditions, etc.) and thereby brings this environment* into equilibrium.

However, equilibrium and disequilibrium are essentially features of human action, which includes as possibilities both error and success. Only in a derived sense are they features of the environment of action. For example, disequilibrium exists when Smith spends all of his income on present consumption and at the same time contracts with Brown to build him a house. On a purely physical level, these actions (Smith's consumption and Brown's building a house for expected payment) are not incompatible as demonstrated by the very fact that they are performed. It is only on the level of choices and intentions that contradictions exist between what Smith does and what Brown does and, consequently, it is only in light of *these* contradictions that the observed physical activities also appear contradictory.

If equilibrium and disequilibrium are essentially features of human action, it therefore follows that both are possible under *all* conceivable circumstances.

[8]Mises (1980, p. 109). See also Mises (1998, pp. 660ff.) and Rothbard (1993, pp. 464ff.).

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At any point in time and at all places, action *can* be successful or in error. Conditions of action therefore can*not* play a role in the question of whether equilibrium does or does not exist. Yet, since action itself is either successful or erroneous—that is, since it is either in equilibrium or in disequilibrium, but never in an intermediate position—"equilibration" is a meaningless expression, a myth that has no place in economic science.

As a corollary, there is no such thing as "arbitrage *ex post*." One cannot import a past state of disequilibrium into a present or future state of equilibrium precisely because both equilibrium and disequilibrium are features of action, not the conditions there of. Action does not involve conditions of the past, only those of the present. Hence, error and equilibrium must also refer exclusively to present action. Insofar as an action is successful (and it *can* be at any time) this action can be described as arbitrage. Yet arbitrage, then, does not connote a comparison between a past and a present state of affairs, but a counterfactual comparison between a given successful action and an erroneous action that could have been performed instead.

A related reason for the impossibility of arbitrage *ex post* is that past errors create the very conditions under which present action takes place, and by reference to which present success and failure must be gauged. The idea that one can resolve past errors by choosing correctly in the present is therefore contradictory and meaningless (see Robinson 1973, pp. 362ff.).

Action and Conditions of Action

Let us develop the foregoing consideration further. The most common reason why past attempts to make sense of equilibrium analysis have failed can be stated in one word: consequentialism. Economists have not taken human choice as an ultimate given, but sought to explain it in more fundamental terms. They have not accepted success and failure as dichotomous elements of economic analysis, but have sought to present them as necessary corollaries or consequences of certain *conditions* of action. Let us briefly state why this is impossible.[9]

A state of equilibrium is characterized by the absence of error. If one claims, for example, that equilibrium is omnipresent, one has to explain why error cannot ever occur in reality. And if one claims that equilibrium merely tends to be achieved under certain circumstances, one has to prove that error cannot—or is less likely to—occur under these circumstances. In any case, one has to prove that error is invariably related to certain conditions of action.

[9]For a general critique of consequentialism and consequentialism in business cycle theory, see Hülsmann (1998, pp. 2ff.).

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If we define choice as a "condition" of action, error is surely related to such a condition. It is a fact that we have to choose—and *this* condition of action never changes. However, it is important to realize that choice cannot be said to *cause* either error or correct judgments. Both success and failure are merely implied in choice as possibilities. In other words, whereas choice is the necessary condition for error, there is no sufficient condition for it: we neither err necessarily nor are able to err intentionally.

Hence, the crucial question is whether error depends on conditions of action besides choice. Only if this were true could one prove that equilibrium would realize itself. If we always choose correctly under certain conditions, equilibrium will be achieved whenever these conditions are present. Are there such conditions? The answer to this question is unambiguous. As long as human beings choose, there is no possibility of proving that we either *cannot* err or *must* err under certain conditions. The idea of a determination of error contradicts the very essence of choice.

Consider the widespread conviction that equilibrium analysis refers to an unchanging state of affairs. The underlying assumption is that there can be no error if conditions do not change. Error frequently occurs, of course, when acting persons have to confront changing conditions. But from the sole observation that one *has* erred in such a case, it does not follow that one *had to* err. Rather, we have to concede that a correct judgment is possible in any situation. Even if the conditions of action were in perpetual flux, we could not deny the possibility that market participants correctly anticipate future conditions. The much-discussed question of whether the real circumstances of action display regular features, or whether they change in a "kaleidic" manner, is therefore irrelevant for economic science.[10]

We see, therefore, that the concept of equilibrium can be meaningful only if it refers to an aspect of *action itself* rather than to a particular state of the *conditions of action*. Only as a category of action can it be given a clear-cut and meaningful definition.

UNDERSTANDING ARBITRAGE AND PSYCHIC INCOME

Let us now turn to the problem of applying equilibrium analysis. The comparative analysis of success and failure gives us a tool for the understanding of reality that is both realistic and *a priori*. We have seen that there is no need to prove that people do not or tend not to err, for such a proof is irrelevant for the

[10]The best introduction to the debate surrounding this question, and an excellent discussion of problems relating to equilibrium economics, is Selgin (1990). For our disagreements with Selgin, see the section entitled "Selgin's Concept of Subjective Profit," below.

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applicability of equilibrium analyses. All possible cases are covered by the theory of equilibrium prices and its comparative counterpart, the theory of profits and losses. This view is realistic because success and failure are potential features of human action. And it is *a priori* because success and failure cannot be perceived on the basis of mere sense impressions and thus cannot be validated or refuted by observations. The problem of applying equilibrium analysis, then, is to *identify* instances of success and failure in the real world.

This problem of identification defies mechanical rules and generalizations. It is the problem of the specific *Verstehen* of historical investigations. Observation allows us to identify two cars running into each another, or a factory closing its doors, but one cannot see an accident or a bankruptcy. Identification of the latter requires understanding on the part of the historian, who must treat each case on its own. In other words, *every instance of success and error must be identified in individual historical cases*. One cannot single out some kind of action and claim that, in general, it is successful or erroneous. Rather, its success and failure must be determined by reference to the individual conditions in which it takes place. Saying "Hi, old chap!" might be the right thing to do when meeting a friend. It would most likely be wrong to do so when introducing oneself to a potential employer. Building a football stadium might be profitable in a prosperous society. It would most likely be a waste of resources if the society went to war and the population starved. To be sure, one cannot explain success and failure *as a necessary consequence* of the conditions of action. Introducing oneself to a potential boss, one *can* do the wrong thing and say "Hi, old chap!" even if we would qualify such behavior as either silly or pathological. Implied as possibilities in choice itself, success and failure are possible under all conceivable circumstances. Man *can* choose the most important of realizable alternatives, but *can* also fail to do so.

However, *once a choice is made*, its success or failure depend exclusively on the circumstances of the individual case. In other words, *before* a choice is made its success or failure is self-determined, and all other conditions of action are irrelevant. *After* the choice is made its success or failure depends exclusively on empirically given conditions. One has to look to see whether the chosen action was indeed the most important one among the realizable alternatives, or whether it rendered impossible the performance of a more important action. Only in the first case could we speak of success; the second would be failure. Applying our *a priori* equilibrium analysis, we make errors intelligible by comparing their implications to the implications of better courses of action that could have been taken. Or, we make successful action intelligible by comparing its implications to the implications of erroneous actions that could have been taken.

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Again, the important point is that equilibrium analysis can be applied only by reference to concrete conditions of the individual case. One has to identify other concrete actions that would have been possible and determine whether they would have been more important. In short, one cannot apply equilibrium analysis by reference to any *a priori* standards. The standard of comparison must be a concrete action that also could have been performed in the same concrete historical situation.[11] We will discuss the problems of identifying such actions in the remainder of this section.

The crucial problem in applying equilibrium analyses is a twofold lack of evidence that concerns both, the value scales of acting persons and the possibility of alternative actions.

These problems are particularly difficult to solve in Crusoe economies—that is, when one analyzes the actions of isolated persons. One can observe Crusoe's actions, but cannot observe his value scales or what he could have done instead of what he did. The only way to deal with such cases is to *guess* what he might have tried to do. Except for instances of what strikes us as pathological behavior, we will have to *assume* that he wanted to produce the effect his behavior brought about, thus assuming what is commonly called "rational" behavior on his part. Still in our interpretation of his behavior, we will sometimes suppose that he failed to pursue the most important project, namely, when we are convinced that he acted against his own rightly-understood interests. We then look at him as a mother looks at her child when it does something that strikes her as stupid, or as a benevolent dictator looks at his subjects when they behave in a manner he deems improper. We could also suppose that Crusoe has failed if *we* know of a better alternative action by which he might have tried to attain the same end that he supposedly attempted. Again, we assure the perspective of enlightened parents or of a higher civilization. There is only one way to establish some (although insufficient) evidence about Crusoe's value scales, namely by relating his currently observed actions to his past actions. If we *assume* that his value scales are by and large stable over time, we can interpret a change of behavior as a discovery of past error (but to be sure, it could also be interpreted as a change of value scales).

These cursory observations help us draw two preliminary conclusions. First, we get a glimpse of just how muddy the waters of such historical analysis

[11]The only other way to approach the problem would be to identify a kind of choice that is *inherently* wrong. The analysis of this kind of choice could run in purely theoretical terms (for such an approach, see Hülsmann 1998). The equilibrium analysis of all other choices must refer to empirical conditions.

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are. Nothing is left of the clarity and apodictic certainty that characterized our theoretical exposition. Although we are certain that there is only one historical truth (because to assume otherwise would be contradictory), we cannot prove that we have captured it. Arguing our perspective on Crusoe's value scales and the alternatives he faced, we cannot refer readers to an objective basis they cannot circumvent. We can only try to understand, and try to make our understanding intelligible. Second, we also clearly see that all assumptions about stability of value scales, "rationality," *homo oeconomicus*, etc. are nothing but crutches for *historical* research. They have nothing to do with economic theory, entering the scene only when necessary to apply economic theorems to understand concrete reality. What economics has to say about equilibrium is apodictically true and, as we shall argue in more detail, highly relevant for a correct assessment of the political significance of profits. However, as far as our understanding of real-world action is concerned, equilibrium analysis merely gives us a few—although valuable—tools. These tools need to be complemented with empirical *ad hoc* assumptions that spring from our personal understanding of each case under consideration.

One can derive still further insight from the case of the Crusoe economy. One of the problems of applying equilibrium analyses, we have stated, is in establishing the value scales of the acting person. We can exclude one view of his value scales from the outset, namely his own view at the moment of decision. The reason, of course, is that at the time he is *convinced* he is performing the most important action. He does not err intentionally, and he could not do so if he tried. Even if someone consciously brought about a "failure," it would be no failure at all. The very fact that the action was intended to produce an ostensible failure implies that it was a success. For example, let us suppose that it is my intention to bring about a failure by jumping from the top of a skyscraper and smashing myself on the ground. If I am smashed, I have not proven that one can err intentionally, for it was my very purpose to be smashed. My action was successful. What we see here is that, as a phenomenon, error can occur only *ex post* an action (see Menger 1871, pp. 21ff.). If the effects of action did not spread in time, all would be present in the very moment of choice. There could be no difference between expectation and reality, we would thus always engage in the best possible action, and there could be no error.

Equilibrium analyses, therefore, cannot fruitfully be applied by referring to the acting person's *ex ante* perspective on his own value scales. However, once a choice is made, any person can meaningfully analyze this choice in equilibrium terms. One does not have to resign oneself to contemplation and wait for all the effects the choice will bring about. Both acting persons and

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outside observers can criticize the choice, pointing out that it is not the most favorable that could have been made. Business observers in newspapers and journals, for instance, do this all the time. Instead of waiting for evidence of the error to manifest itself they anticipate it. This kind of critique is legitimate because error manifests itself *ex post* an action only insofar as it is a *phenomenon*, that is, only insofar as it is evidence of our senses. Objectively, however, error is always manifest in the very action that brings it about. From the sole fact that no acting person *thinks* he errs, one may not infer that there *is* no such thing as error or, to take a more specific example, that markets are always in equilibrium. Thus, error is revealed in *ex post* deviations between plans and reality. But these deviations are not errors themselves, only their manifestations. Error is *committed* in decision itself. As soon as a decision is made, that is, as soon as choice becomes an ultimate given, a legitimate critique can set in and offer recourse to the terms of equilibrium analysis.[12]

Now let us turn to the application of equilibrium economics to analyses of entrepreneurs in a market economy. The first thing to emphasize is that we are still interested exclusively in the success or failure of *individual* actions. It would be groundless to argue that the choices of other market participants determine which of our actions are right or wrong, for this does not affect whether our actions actually *are* right or wrong. General equilibrium is reached when all individuals choose what is for each the most important course of action. We do not have to bother with the question of whether general equilibrium is ever reached, however, as long as we are sure that it *can* be reached.

Fortunately, one can neglect the question of whether other market participants, consumers in particular, act according to their best interests. Their actions are data for the entrepreneur under consideration. He has to adapt his actions to prevailing conditions, and the choices of other market participants are part of these conditions. Analyzing the individual actions of entrepreneurs on the market, we enjoy the considerable advantage market action affords in yielding evidence for valuations and alternatives. When Jackson exchanges ten ounces of gold for Jefferson's car, we can infer that Jackson had the chance to keep his gold or sell it to somebody else, and that Jefferson could have kept his car and put it to other uses. Moreover, we know that Jackson valued the car more highly than the gold, Jefferson valued the gold more highly than the car.

[12]The impossibility of applying equilibrium analysis from an *ex ante* point of view also highlights the methodological division of the social sciences into a theoretical and an applied part. See, on this point, Salerno (1995, pp. 307ff.).

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Most importantly, however, we know that action on the market determines several types of income, and that one such type is profit and loss. On the market, the error of an entrepreneur leads to monetary losses. The returns he realizes for his product do not cover what he paid for capital goods and interest. In other words, the prices he paid for his factors of production were excessive in comparison to his returns, which, in short, constitutes his error. Paying "excessive prices" means that he would have been better off not exchanging his property at all, or purchasing other factors of production and engaging in other enterprises. This way, he would have either realized higher returns, or avoided losses. Similarly, the existence of profits is also an infallible sign of error, for it demonstrates that *other* producers could have done better by engaging in the profitable activity. The existence of profit implies that some market participants would have been better off making other investments, just as the very existence of loss implies that it would have been wiser not to engage in this (or, eventually, any) market transaction. Moreover, it is clear that the market compares the action of the individual entrepreneur not only to the alternatives he considered when choosing, but to the alternatives constituted by the activities of all other entrepreneurs on the market. This is the market's ruthless quest for economic truth. Consumers constantly compare products of entrepreneurs by selecting only the most important ones.

Even on the market, though, evidence for success and failure is not absolutely clear cut. Even in the realm of money calculation, where the categories of wage rate, interest, and profit and loss are especially precious tools, one must *guess* the entrepreneur's value scales, as well as the alternatives he faced, to establish *which* part of his income is profit or loss.

Consider the case of two ice cream dealers selling the same product—which buyers also perceive as the same product—at different prices. The one with the higher income sells in front of a school, the other in front of an old-age home. Let us analyze the impact of value scales on this situation from two sides. First, suppose that the second dealer *hates* teenagers. In this case, as we shall see, his behavior might not involve error. Selling in front of the school would increase his monetary income, but reduce his psychic income, and it is the latter which counts. We might identify error on the side of *other* persons who could have sold ice cream in front of the school, thereby increasing their psychic income. We might also find that there is no other person who might successfully step in to sell ice cream in front of the school at a lower price. In this case, there would be no profit in the present dealer's monetary income. All of his receipts would be wages for his specific labor services.

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Now suppose that the students love the present ice cream dealer. They will buy only his product, and would renounce ice cream altogether rather than buy it from someone else. Again, there might be no profit in his income, only wages. Without reference to the value scale of potential customers, one cannot tell whether an action on the market will represent profit or loss.

Let us now consider the problem of standards of comparisons. The central difficulty is in gauging whether other market actions would have been economically realizable. When analyzing the market, we enjoy advantages arising from the fact that the actions of other market participants sometimes provide the evidence necessary to solve the problem. Consider again the case of our two ice cream dealers. The fact that both sold the same product permits us to say that both could have sold at either place. We can tell the dealer selling in front of the old-age residence, "Look, you could have taken your car and sold in front of the school." Yet, again, this evidence does not enable us to make apodictical judgments, such as in the field of pure theory. For it is possible that no other dealer than the present one *could* sell in front of the school. (The present dealer might be the only one strong enough to defend himself against a gang of nasty schoolboys, for instance.) In this case, there would once again be no profits in his income, merely wages for his specific labor services. Thus, the fundamental difficulty is that we cannot provide clear-cut evidence to answer the question of what the person under consideration might have otherwise done. The very fact that he did what he did prevented him from performing other actions and thereby demonstrating what *he* might have done. There can be no empirical evidence for *his* specific alternatives, because there is no evidence for the counterfactual.

Apart from this problem, there are questions as to which kind of alternative action should form our base for comparing market actions. Should it be an action that the decision maker considered at the moment of choice, or should it be *any* better action, even one he did not think of when choosing? Consider the case in which two groups, while ignoring each other, exchange the same good at different prices. Is this a case of error, or not? According to Stephen Shmanske (1994, p. 210), "this market is only in disequilibrium with reference to some perfect information benchmark; this perfect information does not exist in the hands of the relevant actors in the market and, therefore, is irrelevant." Shmanske concludes that the market is in equilibrium – that is, that no error can be identified. Israel Kirzner (1985, p. 158f.), by contrast, sees here a case of disequilibrium. Who is correct?

Remember that we can use the distinction between success and failure as an analytical tool for comparison. It is obvious that, in the case cited above, the

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group selling at a lower price *could* have sold at a higher price somewhere else. We *can* therefore meaningfully compare their actual actions to actions they did not consider when making the choice. This is a common practice of daily life. With the benefit of hindsight, we look at a choice and compare what has happened to what could have happened if we had made other choices. We might be able to "forgive" ourselves more easily if we look back convinced that we did not even think of other actions at the time. (As an outside observer, one of course has the additional difficulty of finding evidence that the alternative really was considered.) However, this does not change the fact that one *can* meaningfully compare past actions to alternative actions that were ignored at the time of the decision. A completely different question revolves around *which importance* should be accorded the errors we so identify. Everyone might judge for himself whether this kind of comparison is relevant or not.

In retrospect, one always finds evidence that puts past decisions in a new light. Although in many cases it might be difficult to say whether our past actions have indeed been successful, such difficulties do not at all invalidate the fact *that* there is always a best or optimal action. They often stem from the reality that not all effects of our actions have *as yet* become visible. We often have to wait to see whether our past judgment was or was not the best possible. If we wait long enough, we shall always be in a position to gather evidence to gauge whether or not we chose the most important action. For example, investments that at first seemed to be ruinous can eventually realize important returns. And even the most initially promising enterprise might go bankrupt because of unforeseen events. If, looking back, we find no fault with our past decisions, if we find that we always chose the best option possible, our life has been optimal. And if, in retrospect, we discover errors, we are only capable of identifying them because we can conceive of a better alternative that we could have realized instead.

The fact that the future might produce new evidence for and against the success of past endeavors implies that the standards of comparison by which we gauge such success are constantly being modified. What was formerly considered the best option now seems only second-best, or, in other words, wrong. We see it as wrong *now* because we realize that carrying it out prevented the execution of a more important alternative. What does this imply for the writing of history, as far as history is an application of economics? It implies that history must of necessity be "revisionist." It criticizes our old view of what was right and wrong in light of the new evidence. Although we always employ means that, in our *ex ante* judgment, realize the most important end, we sometimes discover *ex post* that another course of action would have been more favorable. We then see that our *ex ante* judgments deviated from what events

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now show us was reality. This deviation is the manifestation of error. *Ex ante*, we always choose what we think is the most important option. *Ex post*, we compare what is with what would have been, and thereby discover our errors.

It would be groundless to object that this conception of success and failure is too restrictive, that it would lead to every plan being thwarted except that made by a clairvoyant or a very lucky planner. As should be clear from our foregoing discussion, equilibrium is nonetheless realistic, and nonetheless important for economic analysis, even if nobody attains it.

The purpose of the foregoing discussion was to highlight the intricacies of applying equilibrium analysis and to contrast it with the result that this kind of research can bring. Instances of proper applications can be found in investment newsletters and business reports as well as in biographies of business executives and other leaders. Let us emphasize that these applications do not add one iota to the *political* debate surrounding profit, income, and distribution, however. This is not because applications of equilibrium analysis refer to individual cases instead of to the economy as a whole, but because applications themselves teach us nothing about the nature of profit, but rather use this category as a tool. Theory, not historical applications thereof, must guide us in political decision making.

THE ANALYTICAL AND POLITICAL SIGNIFICANCE OF EQUILIBRIUM

Let us now briefly examine the analytical and political significance of equilibrium economics. As we shall see, our realist approach underscores precisely the views Mises held on the role of equilibrium in economic science.

First, let us recall that equilibrium analyses do not give more or less accurate pictures of reality. If this were their purpose, they would not be relevant to our understanding of the world. They are relevant because they enable us to understand our world through comparisons with the counterfactual, and because the counterfactual is implied in the choice under consideration. All human undertakings contain both success *and* failure as possibilities. Equilibrium analysis not only encompasses both the possibility of success and of failure, but *consists of* a comparison of the two.

Second, equilibrium analysis is only a *part* of economic science, and therefore must not be equated with it. One can discuss most issues of relevance to economic theory and policy on grounds other than those of equilibrium analysis. Virtually all issues relating to monetary theory and policy, for instance, can be discussed by reference to the nature of money, and inflationary schemes can be rebutted by pointing out, in various contexts, that more paper money does

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not increase factors of production. Or, socialism can be criticized for lacking the *possibility* of monetary calculation (that is, independent of the question of whether monetary calculation always brings about equilibrium). The very existence of such other fields of analysis proves that equilibrium analysis is just one part of economic science.

What, then, is the specific task of equilibrium analysis? It is limited to—and necessary for—the determination of income streams on the unhampered market. Here it performs two important tasks. On the one hand, it enables us to determine the relative height of wages and interest. Without the notion of equilibrium, we would know only *that* and *why* labor services command a price on the market, and *that* and *why* rents paid for unit services of land and capital represent interest on the present value of these goods. Yet only equilibrium analysis shows us, for example, that interest must be uniform throughout the economy, or that wages correspond to the discounted marginal value product of labor services. On the other hand, equilibrium analysis enables us to distinguish a third kind of income, namely profit and loss. It shows that profit and loss represent a residual income, and that this income springs from error. These are the main theoretical propositions of this branch of economic inquiry.

The politically relevant implications that one can derive from equilibrium analysis concern schemes for income redistribution. Demonstrating the residual character of profits and losses, for instance, can be used to vindicate just such income. It is obvious that the entrepreneur who makes profits must not be blamed for low wages, for, without him, wages would be *even lower*. His profits spring from omissions of other entrepreneurs who could have realized higher incomes by bidding underpaid labor away from him.

Moreover, because profits and losses are the result of error, they cannot be "abolished" by government intervention. Irrespective of who controls the means of production—be it a central planning administration or private property owners—one cannot deny that they commit errors. Government interference may provide for a redistribution of profits and losses different from what would have been occurred on the free market. Burdening the taxpayers, government may help incapable entrepreneurs who otherwise would have had to pay for losses out of their own pockets. However, no government (or anybody else) can create schemes that preclude error, which is a fundamental feature of human nature.[13]

[13]Also, no government can "tax profits away." The distinction that equilibrium analysis establishes between profits and other kinds of income cannot be readily ascertained in the incomes of market participants. Looking at the annual income of a an opera singer, one

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Many economists believe that equilibrium analysis not only serves to evaluate schemes for income redistribution but to evaluate institutional settings in efficiency terms. The idea underlying this view is that some institutions might be more able than others to bring about equilibrium. The former institutions are then said to be more efficient, the latter less so. Because the conditions of action determine success and failure—so the reasoning goes—the task of the economist is to identify the set of conditions that creates the best outcome, thus making the world safe for success and efficiency. This view has already been severely challenged.[14] Indeed, there are two decisive criticisms.

First, to act successfully means to act successfully under *given* conditions. Success is not an absolute detached from the environment of action. It is relative. Successful actions are those best suited to present and future conditions, *whatever these conditions are*. From this, it follows that equilibrium can exist under any institutional setting.[15] Equilibrium can exist in an individualist as well as a totalitarian society. By modifying the institutional setting in which human action takes place, one does not increase efficiency but the terms in which it is gauged. Thus, if an accountant spends his time watering his boss's flowers instead of doing his job, this might be highly efficient in a totalitarian system, but would be a waste of time in a free market. Watering the flowers in the first case, and abstaining from doing so in the latter, he could be said to act successfully in both cases. In this context, it can hardly be overemphasized that Mises's economic calculation argument, which claimed that socialism lacked the indispensable tool for a rational allocation of resources (namely, market prices), and which brought about the comparative evaluation of economic systems, was not cast in equilibrium terms. Mises argued that socialism *lacked* something present in capitalism, something indispensable to the allocation of resources. He did not argue that capitalism was more likely to reach equilibrium.

The profession did not choose to follow Mises, however, but pursued the efficient-outcome path to its logical dead end. Many economists will probably

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cannot tell which part of this money stream is "profit" and which is payment for his specific labor services. It is therefore not surprising that, when seeking to "tax profits away," the practical solution does not consist in taxing profits but in taxing income. Yet, in this case, the taxes become a part of the entrepreneur's costs of production, and are incorporated into his calculations. And profits (and losses) remain what they were, namely residual income components that add to the so-calculated income.

[14]See, for example, Schumpeter (1908, pp. 196ff.) and Buchanan (1979 and 1969). The specific Austrian approach to the analysis of socialism and interventionism, moreover, is an implicit challenge to this widespread view.

[15]The only conceivable exception is, again, that these institutions are inherently erroneous. See footnote 11, above.

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be unsatisfied with the way we presented equilibrium analysis, namely, as dealing with *individual* success and error. However, the comparative advantages of this approach are too obvious to be overlooked. It is both logically impeccable and meaningful to speak of individual success and error in any concrete historical setting. By contrast, it is not meaningful to speak of the "success" or the "efficiency of the system," since success and failure are categories of action, and action is always *individual* action.

It is meaningless to ascribe individualistic terms to some aggregate entity. There is, for instance, no such thing as an absolute scale of values by which economic progress or regress of society can be gauged. To perform economic calculations, we must compute market exchange rates (prices). Yet, this presupposes *two* owners who have *different* views about the good, lest no exchange would take place and no exchange rate could be established. How, then, could one possibly estimate the value of a single good from *one* point of view (the point of view of "society as a whole")? Indeed, this is entirely impossible.

Moreover, when it comes to money prices, what entrepreneurs calculate is the profitability of possible investments. These calculations serve to *compare* these investments with one another to identify the best course of action. No other meaningful use can be made of market prices.[16]

The second criticism of the attempt to apply equilibrium analysis to institutional settings in efficiency terms focuses on the consequentialism of this approach: the attempt to identify the institutional setting that brings about equilibrium presupposes that success and failure are consequences of the conditions of action.

This fallacy is strikingly present in all claims that equilibrium is omnipresent (which, politically, implies an economic justification of the *status quo*). Many modern economists, for whatever reason, have failed to notice that equilibrium analysis is essentially a *comparative* study, and that encountering economic error is an integral part of it. Instead, they have focused on the success-side of such analysis and interpreted it as a more or less faithful picture of the world. In their eyes, the more reality *conforms* or tends to conform to equilibrium, the more meaningful equilibrium analysis becomes.

As a consequence of this misapprehension, many economists tend to regard error as merely a disturbing feature of reality. They believe that the existence of error reduces the importance of equilibrium analysis. Therefore, they try to demonstrate that error is a minor phenomenon, and explain why this is so. All such attempts fail, and necessarily must, because one cannot

[16]For further discussion of the problems of economic calculation, see Hülsmann (1996, pp. 133ff.).

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explain in a general way why error comes about. All who assume this line of reasoning fall prey to the consequentialist fallacy.

One can divide the various consequentialist views on equilibrium analysis according to the conditions from which equilibrium allegedly must follow. The most widespread assumption is that equilibrium ensues whenever conditions do not change any more.[17] Marshall assumes that equilibrium results when changes are sufficiently quick or sufficiently slow enough not to affect analysis (see 1920, p. 307), or when market participants can recontract (see p. 335). Other authors consider equilibrium the consequence of market participants enjoying rational expectations (see, for example, Miller 1984), or of error being negligible (see Walras 1988, pp. 11, 110; Edgeworth 1961, p. 12). Still others believe equilibrium characterizes a world lacking entrepreneurial activity (see Wicksell 1934; Schumpeter 1911); one that does not generate messages that cause

[17]The conviction that equilibrium implies an absence of change is and was common to most modern economists. Marshall (1920, p. 305), on the stationary state: "in it the general conditions of production and consumption, of distribution and exchange remain motionless." Clark (1925, p. 28) observed the following about the static state:

> It is conceivable that production might go on in an organized way without any change in the character of the operation. Men might conceivably produce to the end of time the same kinds of goods, and they might do it by the same processes. Their tools and materials might never change; and they might not alter, either for the better or for the worse, the amount of wealth that industry would yield. Social production can thus be thought of as static.

Fetter (1915, p. 130) wrote the following about equilibrium:

> Where this best proportion is attained, is a point of economic equilibrium in the sense that there is in the situation itself (and until some other conditions change, as invention, increased demand, etc.) no motive to change the proportion. In such a case the effort is made to repeat the process, to maintain just that proportion which has been found to be on the whole best.

Pareto (1966, p. 153) held that: "economic equilibrium is the state of affairs that would maintain itself indefinitely if there were no change in the conditions under which it is observed" (my translation). Böhm-Bawerk (1968, p. 412f.) speaks of "Dauerpreis" (permanent price) or "dauernder Stand der Preise" (permanent level of prices). Mises's (1935, p. 109) thoughts on the matter seem to be based on the Clarkian conception of a static state: "The static state can dispense with economic calculation. For here the same events in economic life are ever recurring." See also Mises's (1939, pp. 106ff.) statements about the "économie immuable." Speaking of the "evenly rotating economy," Mises (1998, p. 247f.) says that it is characterized by "the elimination of change in the data and of the time element. . . . All factors, including those bringing about the recurring disarrangement of the plain state of rest, are constant. Therefore prices – commonly called equilibrium or static prices – remain constant too." On the ERE, see also the section "On Mises's Evenly Rotating Economy," below. An interesting case is that of Hayek. In an early German-language paper, he (1928, p. 38f.) advocated perfect foresight or perfect knowledge as the condition necessary for equilibrium. Then, in the middle of the socialist calculation debate, Hayek (1935b, p. 212) apparently changed his mind: "We should not expect equilibrium to exist unless all external change had ceased." For a good overview of the manifold ways economists have conceived of static equilibrium, see Machlup (1963, pp. 13ff.).

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agents to change their views and ideals (see Hahn 1973, p. 25); or one without market prices, which operate as road signs toward success.[18] A more recent instance of consequentialism in equilibrium analysis is the argument that markets dominated by big players will provide less reliable expectations (see Butos and Koppl 1993, pp. 302ff.).[19]

A CRITIQUE OF OTHER APPROACHES TO EQUILIBRIUM ANALYSIS

On Equilibrium Selection in Game Theory

Although game theory is a comparatively recent approach, it is useful to deal with it first. Its perspective on equilibrium analysis is different from all others in that its primary aim is not descriptive but prescriptive. Game theory sees equilibrium as the "solution" to a "game" that *should* be chosen by rational people taking part in it.[20]

This normative orientation is in itself an elegant solution to the crucial problem posed by the relationship between equilibrium economics and observed reality. Insofar as the purpose of economics is to enable an observer to *describe* reality, one must account for the relationship between economic theory and the observed real world. While all other traditional approaches to equilibrium analysis deal with this problem, albeit inadequately, game theoreticians consciously and elegantly sidestep it.[21] They neglect the question of how people behave and focus instead on how people should behave. They analyze constellations of human interaction (games) in order to unearth the

[18]For a critique of the road-sign theory as embodied in the contemporary Hayek–Kirzner theory of the market process, see Hülsmann (1997). On equilibration, see also Hicks (1965, chap. 2), who claimed that comparative statics consist in the comparison of "any basic process" and an "amended process."

[19]From these consequentialist fallacies one must distinguish imprecise uses of language. Thus, according to the young Hicks (1946, pp. 133ff.), the causes of error are inconsistencies of price expectations, inconsistencies of plans, incorrect forecasts of wants, and cases in which only second-best solutions are pursued from the outset. These are obviously not causes, however, but manifestations of error. The same fallacy is present in Hicks's (1946, p. 254) claim that instability is "explained" by the assumption that people start doing silly things like giving unlimited credit, etc. Similarly, the "conditions" of equilibrium that Hicks (1946, pp. 86, 197) enumerates are manifestations or characteristics of equilibrium.

[20]See, for example, Neumann and Morgenstern (1944), Nash (1951), Damme (1987), Harsanyi and Selten (1988), and Baird et al. (1994). Game theorists use the expression "rational" in its colloquial sense of "what a smart person would do" or "what is objectively suited to attain a given end." By contrast, Austrian economists cherish a subjective notion of rationality by virtue of which human beings always act rationally, since they believe that the means they employ are appropriate to their desired ends.

[21]Awareness of this problem can be found in works of the pioneers of game theory. See, in particular, Morgenstern (1928, 1934, 1935, 1937), and Neumann and Morgenstern (1944).

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best strategy for each player, thus determining the likely outcome when rational players play them. As we have said, this is an elegant approach since, conceivably, an ideal can be right or wrong even if it does not correspond to anything that exists in the actual world—provided that it can, possibly, be put into action.

Before we set criticize the game-theory approach to equilibrium analysis, let us observe that it does not directly contradict the realist approach advocated in this article. We have dealt with equilibrium analysis here as a descriptive tool, primarily, and discussed its role in the theory of economic policy. We have argued, that in each setting, a unique equilibrium exists—a position also emphasized by several champions of game theory (in particular, Harsanyi and Selten 1988). And we were able to neglect the question of whether it is possible to determine the equilibria of concrete forms of interaction (games) *for all times* since the crucial point for us was that there simply be a unique equilibrium. One could be inclined to believe, therefore, that the two approaches are complementary. Game theoreticians could develop standard solutions for various constellations of human interaction, which we could then apply in counterfactual descriptions of success and failure in the real world.

Assessing the prospects of such a division of labor, we have to examine the question of whether games can be so formulated that they precisely describe real-life conditions of action. If this were not so, it would be impossible to discern to what degree the "solutions" of game theory are relevant to real human life. They would then be solutions just as any arbitrary utopia is a solution instead of in the sense that the best concrete alternative is a solution. The postulate that games be so formulated that they exactly fit corresponding features of the real world does not, of course, mean that a game must somehow reflect *all* features of the real world. Rather the question is whether its constitutive elements—that is, those from which the results of game theory derive—adequately correspond to certain real-world features of action. With this in mind, we will now examine the following constitutional elements of game theory: the number of players, strategies, and rewards.

There should be no problem as far as the number of players is concerned. A two-person game, for instance, is applicable to all situations in which two persons interact in the manner determined by the game.

The matter is entirely different when it comes to strategy, which in game theory is defined so broadly as to imply actions taken to attain a given end. The problem here is that game theory postulates the possible strategies of players as a given. More precisely, it postulates that in each game, all possible strategies are defined from the outset. This can take the form of an explicit statement

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of each strategy (for example, confessing a crime or not confessing a crime) or a definition of the boundaries within which strategies might be chosen (for example, drawing any number between 0 and 1). Most game theory analyses found in the textbooks assume that there are only two possible strategies.

There are games in which all possible strategies can be defined at the outset—but, not surprisingly, only children or fools can play them for any considerable length of time. The characteristic feature of such engaging games as chess, tennis, or boxing is that they allow for the application of countless strategies unknown at the outset. And, when it comes to real life, there are unlimited possible but unknown strategies, for human creativity constantly overthrows old patterns, adding new strategies previously unimagined. This fact prevents the identification of something like a timeless solution to problems of human life. Game theory can handle only those strategies the analyst himself can imagine. Yet, when someone conceives of what nobody thought before, and puts these ideas into action, we would have to confine the former "solution" to the dustbin of history.

We encounter even greater problems once we turn to rewards. In most expositions of game theory, rewards are *physical* consequences of the various modes of interaction between players—for example, sums of money that player A receives when he performs action x and player B performs action y.[22] Game theoreticians make the (mostly tacit) assumption that these rewards are *per se* desirable (rather than being desired by a person), and that the degree of their desirability can be expressed in terms of their physical characteristics. For

[22] Some of the following arguments will focus on this type of exposition since it appeals to common sense and is largely responsible for the recent success of game theory. In the original exposition in Neumann and Morgenstern (1944), as well as in some strands of later literature, rewards are *cardinal utilities* that obtain as a consequence of the various modes of interaction. Most game theorists would probably deny that this approach is subject to the standard criticism leveled against the notion of cardinal utility, in particular the criticism that one cannot compare cardinal utilities of different persons (see Rothbard 1956). They would point out that cardinal utilities of rewards in game theory are derived from a purely ordinal ranking. The rewards are ranked, in the words of Baumol (1958, p. 666), "against an arbitrarily chosen imaginary lottery ticket which is used as a standard of comparison." The player is supposed to interpret each reward as a "compound lottery ticket" and to evaluate it "in terms of the probabilities of winning the ultimate prizes" (p. 670). However, first, a ranking in terms of some prizes quickly leads us back to physical rewards, and thus to criticism we will point out below. Second, this ranking does not encompass those rewards that are more highly valued than the total lottery prize, as well as those that are valued less than the participantion in the lottery. Third, any real-life lottery faces the following problem. Either the total sum of prizes is fixed beforehand—then it is *uncertain* whether the lottery can be successfully carried out, and the standard of our ranking is no longer fixed. Or the sum of prizes depends on overall participation in the lottery—then the standard is uncertain, too. Fourth, and finally, Baumol's utilities are gained through a purely intellectual exercise. Their empirical determination would require such a ranking to be actually carried out—but this would entail the problems we just mentioned.

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example, 100 dollars is preferable to 80 because it is a larger sum, and one year in prison is preferable to ten because it is a shorter term, etc.

There are light years, obviously, between this way of evaluating players' strategies and the approach known as subjective value theory. But, since this is not the place to go into great detail, we will point out only a few of the most significant shortcomings of this approach.

First, it does not cover the overwhelming majority of cases in which the rewards are heterogeneous. This is no birth defect of game theory that could be remedied by improvements to the approach, but an essential feature that makes it what it is. In each game, all rewards must be of the same kind, for their ranking would otherwise would be too obviously arbitrary . It is plausible to argue that 100 dollars is always and everywhere preferable to 80. It is far less plausible to contend that 25 bananas are always and everywhere preferable to a rib-eye steak. For the same reason, game theoreticians take into account only rewards that most people find either desirable or undesirable, such as money or prison terms. It is plausible to assume that everybody likes money—any quantity of money—just as it is plausible that no rational person likes prison terms of any length whatsoever. It is far less plausible that everybody always and everywhere prefers more perfume to less.

Second, the game-theory view of physical rewards does not account for such widespread phenomena as charity. People who deliberately renounce a bigger sum of money in favor of a smaller one are branded as "irrational," which simply means that they contradict game theorists' preconceived notions of what it means to be rational.

Third, and most decisively, the strategies themselves are never considered part of the rewards—which they are in virtually all real-world cases. In game theory, rewards can only be events that happen as a consequence of players' actions. It is not allowed that a player prefers a given strategy for its own sake, say because it is beautiful or ethically satisfying. Engaging in scientific research because it is gratifying, of instance, is not allowed. Game theory considers any behavior rewarding only insofar as it yields a rewarding result different from the behavior itself. All other considerations are excluded—only instrumental reason is "rational."

Again, this is no accidental defect but an inherent feature of game theory's very enterprise. Homogenous rewards in game theory are to serve as standards by reference to which the heterogeneous strategies of players can be evaluated. Rewards must be homogeneous, for, otherwise, strategies could not be compared in the same terms. Strategies can never be homogeneous by virtue of the fact that they are different. If a strategy were desirable for its own

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sake, it could no longer be compared to other strategies in terms of the same physical standard and the whole delicate edifice of game theory would crumble.

These considerations suffice to demonstrate that game theory does not and cannot reach its self-chosen purpose of selecting timeless equilibria. More generally speaking, game theory is unsuited for the scientific analysis of human action.[23] It is an intellectual pastime of university professors and their students that remains genuinely unrelated to the real features and problems of human life.

On Profits and Prices in Knightian Equilibrium

Past approaches to equilibrium analysis, insofar as they conceived of equilibrium as a tool for the descriptive analysis of the real world, suffered from one of two shortcomings. Either they were noncomparative—that is, constructed upon the idea that equilibrium analysis could only be relevant if and insofar as equilibrium was realized in the real world, or they were comparative, but did not identify the correct elements of comparison.

The latter deficiency is manifest in F.H. Knight's very influential view on equilibrium. In his seminal work *Risk, Uncertainty, and Profit*, Knight claimed that equilibrium is a feature of a world of perfect foresight. This world is characterized by the existence of risk and the absence of uncertainty. Because the market participants can weather risk, no profits or losses exist in such a world. By contrast, uncertainty prevails in the real world, and uncertainty defies perfect foresight. Therefore, we observe profits and losses.

This conception of the problem at hand was taken up by virtually the entire profession. It provides, in principle at least, the basis of the twentieth century's economics of profit and equilibrium. Knight's specific distinction between risk and uncertainty has since been challenged and modified, but his fundamental idea is still alive. The idea is to explain why, and under which conditions, there is error on one hand and certainty on the other, and to gain insight into the workings of our error-ridden world by comparing it to one of certainty.

[23] In a recent article, Nicolai Foss (2000) argues that game theory and Austrian economics are compatible and therefore well-suited for cross-fertilization. Foss claims that, from an Austrian perspective, the main insufficiencies of game theory are its formal character, its equilibrium orientation, and its assumptions about the knowledge players possess. Yet these difficulties can be resolved, and as a result, one should expect an improvement of both Austrian economics and game theory. However, even if we admit for the sake of argument that the Austrian approach is less formal than other approaches, that equilibrium plays no role in Austrian economics, and that knowledge, learning, and discovery are important Austrian themes, the gulf between game theory and Austrian economics is still unbridgeable. The whole program of game theory—selecting standard solutions—utterly fails, for it cannot cope with the fundamental facts of human life we have discussed above.

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On Knight's lasting contributions, George Stigler (1985, p. x) writes the following:

> profit, which of course may be negative or positive, arises only when there is uncertainty in the outcome of the productive process. When and to the extent that events are predictable individually or *en masse*, they give rise only to wages or rents (including risk premia).

In the Knightian conception, profits and losses are ultimately bound up not with choice, but with the circumstances of action. It is not that man errs, but that events are "unpredictable." In short, uncertainty is a feature of the conditions of action that *imply* error. No equilibrium *can* exist in an uncertain world. Only in the never-never land of certainty could all opportunities for arbitrage be utilized and all profits and losses disappear. Accordingly, today's dominant interpretation of profits is that they are "the outgrowth of uncertainty" (Rothbard 1993, p. 465), whereas equilibrium is considered the outgrowth of certainty. Among economists, there are in this regard differences of opinion concerning only two questions: on the one hand, *which* conditions of action create certainty and uncertainty, and on the other, the universality of these conditions.

The offshoot of this view was a permanent separation of equilibrium from the real world. Equilibrium economics refers to the never-never land of certainty. It is only indirectly meaningful for our world, and it is still not clear exactly what "indirectly" means. Not surprisingly, this conception has proved fatal for the reputation of economics among the broader public, and for the development that our science has since taken. Laymen and students learned that economics dealt with equilibrium, but that equilibrium had nothing to do with our world. What conclusion could they possibly draw from such a view? It is not our task to rewrite the history of twentieth-century economics under the influence of Knightian nihilism. We have to deal exclusively with its analytical significance, and in this regard we have to make three points.

First, there is no such thing as a certain world that can be meaningfully distinguished from our real, uncertain world. Any world relevant to economic science is peopled by human beings, human beings are free to choose, and this this very freedom of choice that defies any attempt to determine "laws" of what they choose.[24] Lacking of such laws, everybody is confronted with the inescapable fact of uncertainty.

Second, there is no need to separate equilibrium from the real world. Equilibrium is constituted by successful action, and action *can* be successful

[24]See Rothbard (1997, chaps. 1–6), and Hoppe (1982; 1989, p. 112f.; 1993, chap. 7; 1995, pp. 36ff.).

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under any circumstances. Therefore, one does not have to postulate that uncertainty is absent in equilibrium. We must phrase our arguments about equilibrium in a comparative manner, that is, by comparing it to the features of error, another potential feature of real action. This comparative approach to equilibrium economics is applicable to each instant of our real world.

Third, Knight's approach was not only vitiated by the assumption of perfect knowledge. His conception of equilibrium marked a return to the holistic approach of many of the classical economists. He did not address individual actions, but an "imaginary society" the members of which are "a 'random sample' of the population of the industrial nations of today" (1985, p. 76). He argued that theory modeled or represented an actual or imaginary society. As a consequence, he took recourse to those contestable assumptions that sometimes are used in historical research. Thus, he (1921, pp. 76ff.) assumes that each member of his imaginary society acts "in response to real, conscious, and stable and consistent motives, dispositions and desires," that "nothing is capricious or experimental," that he "controls his own activities with a view to results which accrue to him individually," that he "is to act as an individual only, in entire independence of all other persons," and that "productive operation must not form habits, preferences, or aversions."

If Knight were right, one would have to conclude that economics is much more restricted than it actually is. A student getting acquainted with economics via the Knightian approach must find a concept like profit, and the determination of the height of wage and interest rates is contingent to particular conditions—*very* particular conditions.

Yet, whereas these conditions might obtain under certain rare circumstances of time and place, Knightian "perfect equilibrium" also requires conditions that, like perfect knowledge, are *never* given. Thus, Knight (1985, p. 77f.) presupposes the "complete absence of physical obstacles to the making, execution and changing of plans at will"—that is, that there is "no cost involved in movement or changes," that "all the elements entering into economic calculation . . . must be continuously variable, divisible without limit," and that there prevails a "continuous, costless intercommunication between all members of society." These statements by an outstanding practitioner of economic analysis were instrumental in spreading the conviction that the meaning of economic doctrine depends upon just such assumptions. Yet their true purpose is to prepare a holistic (and completely unhistorical) model of reality.

Based on this account of economic science cannot answer the pressing political questions of mankind. To the question, "What determines my wage rate?" Knightians cannot just answer, "the discounted marginal value product

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of your labor." They must also add the proviso, "*provided* that all goods are perfectly divisible, that you and all other members of society are rational and omniscient, and that neither you nor any person or good encounters physical obstacles." Is it any wonder that people do not listen to what economists tell them about the determination of incomes? Knight and his followers thus fall under the verdict of one of the great masters of our science, Jean-Baptiste Say, who wrote the following about such attempts to model reality:

> Such persons as have pretended to do it, have not been able to enunciate these questions into analytical language, without divesting them of their natural complication, by means of simplifications, and arbitrary suppressions, of which the consequences, not properly estimated, always essentially change the condition of the problem, and pervert all its results; so that no other inference can be deduced from such calculations than from formula arbitrarily assumed. (1971, p. xxviin)

On Mises's Evenly Rotating Economy

Mises's equilibrium construct, the "evenly rotating economy," is another instance of a comparative approach to equilibrium analysis that fails to identify the correct elements of comparison. Whereas in Knightian equilibrium acting man *knows* what will happen in the future, his colleagues in the ERE are, in Mises's (1998, p. 249) words, "soulless unthinking automatons." They do not act. They react—mechanically and uniformly—to conditions that manifest themselves again and again. The ERE is thus characterized by

> the elimination of change in the data and of the time element. . . . [It is] a fictitious system in which the market prices of all goods and services coincide with the final prices. There are in its frame no price changes whatever; there is perfect price stability. The same market transactions are repeated again and again. . . . The system is in perpetual flux, but it remains always at the same spot. It revolves evenly round a fixed center, it rotates evenly. The plain state of rest is disarranged again and again, but it is instantly reestablished at the previous level. All factors, including those bringing about the recurring disarrangement of the plain state of rest, are constant. Therefore prices—commonly called static or equilibrium prices—remain constant too.[25]

[25]Mises (1998, p. 247f.). Before Mises, Hayek (1928) had pointed out that equilibrium, or the state of rest, was an imaginary construction, that is, a tool of economic analysis and not a description of reality. In his eyes, prices are "the guides and regulators of all economic activities" (p. 34), and, since equilibrium therefore tends to be reached, equilibrium analysis makes it possible to summarize (*zusammenfassende Darstellung*) a great number of different tendencies in the economy (see 1928, p. 38). In a slightly different account of the function of equilibrium analysis, Hayek (1928, p. 39) claims that the results of equilibrium analysis are relevant only insofar as future changes in data are known (*bekannt sind*).

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The ERE can avoid the uncomfortable assumption of perfect knowledge by postulating that the conditions of action do not change. The evenly rotating market participants are not omniscient. They do not have perfect knowledge of the future. What the ERE presupposes is that, *given any knowledge* of technology and of the particular circumstances prevailing in the market, there will be a tendency toward equilibrium as soon as circumstances and knowledge (a part of the conditions) stop changing. The result is a neat picture of the market process: If conditions stop changing, sooner or later only those enterprises most important under these conditions will survive. All other enterprises will be given up *because* there are other more important enterprises. All less important enterprises will in fact become unprofitable because the more important ones, which will realize higher receipts, will be able to pay higher factor prices, thereby ever increasing the costs of less important firms until they incur losses. Because nobody can incur losses indefinitely, sooner or later only the most important firms will survive. The economy will have arrived at its "final state of rest," and turns into an ERE.[26]

Mises claimed that the ERE is "both appropriate and indispensable" to analysis of the market process. More precisely, he found it necessary to treat "the problem of the relation between the prices of products and those of the factors required for their production, and the implied problems of entrepreneurship and of profit and loss."[27] But why is the ERE appropriate and indispensable for these endeavors? Mises's answer is highly significant. He sees the ERE as an instrument of a more general method of economic investigation. Its function is not merely to explain entrepreneurship and profit and loss, but to "comprehend in what respects the conditions of a living world in which there is no action differ from those of a rigid world. This we can do only by the *argumentum a contrario* provided by the image of a rigid economy."[28] He thus adheres to the Knightian method of contrasting our world with an avowedly unrealistic, imaginary construct.

Mises saw more clearly than Knight, though, that the idea of equilibrium is only one part of a comparative investigation. He was very conscious of the fact

[26]This view seems to have been current among Viennese economists of the 1920s and 1930s. See also Conrad (1936), Strigl (1934, p. 89), Rosenstein-Rodan (1927, p. 1206), and Weiss (1923, p. 16).

[27]Mises (1998, p. 249). In an earlier essay, he (1939, p. 110) wrote the following about the purpose of the ERE, that it is: "the study of the relationships that exist between prices and costs, and, consequently, of entrepreneurial risks" (my translation).

[28]Mises (1998, p. 251). Consider also Lindahl's (1939, p. 34) quite similar position: "we need the static structures as instruments of analysis. If we can state under what conditions the variables studied do not change, we can better understand the course of their actual fluctuations."

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that the validity of economic theorems did not presuppose the fact that equilibrium exists. He clearly saw that the concept of equilibrium is but a tool of analysis. It does not matter whether conditions will ever be stable and, consequently, whether the ERE will ever be achieved. Its function is to serve as a standard of comparison from which we can derive insights about the implications of unstable conditions.

However, neither Mises nor any other advocate of the ERE has denied that this concept is rife with contradictions. One cannot imagine, for example, a demand for money in a world of certainty. It would be senseless for the market participants to hold a medium of exchange, for all exchanges could be effectuated in kind. Another problem concerns the fact that some resources can be depleted.

The crucial point, however, is that even unchanging physical conditions do not independently bring about equilibrium on the market. Equilibration toward the ERE is supposed to operate according to the view of the market process as outlined above: in time, only the most important firms will survive. It is important that, according to this view, firms driven out never come back, while firms later to be part of the ERE exist at the outset. The equilibration process uncovers which firms out of many are the most important. Only these firms survive in the equilibration process and arrive at the "final state of rest," which then reproduces itself endlessly, thus becoming an ERE. However, it must be noted that one must count the distribution of wealth among the supposedly stable conditions of action, and that the market process always leads to a redistribution. Whenever a firm is driven out of the market, its employees will have to work elsewhere, accepting lower wages. Other members of society, the immediate competitors of this firm, for instance, will realize higher incomes. (What will they do with the money? Are they already robots or is there still some entrepreneur in them? We find no statement in Mises as to when the persons living in this equilibrating world will become the automatons they are in the ERE. Let us suppose, for the sake of argument, that they are still entrepreneurs.) If these beneficiaries of the redistribution process do not use their increased income in precisely the same way as before, a new set of market conditions will obtain. Firms that before were among the most important will become submarginal. Firms that before incurred losses will become profitable. There is no reason to assume that the same firms will endure any economic process, even under stable, physical conditions. Therefore, the conditions under which the final state of rest is supposed to be established—namely, *stable* conditions—can never be given. The market process itself brings about continual change, it implies a constant need for readjustment. Under such circumstances, only the

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successful actions of entrepreneurs can establish equilibrium. It might be this fact that prompted Mises to state that

> It is even out of the question to carry the imaginary construction of an evenly rotating system to its ultimate logical consequences. For it is impossible to eliminate the entrepreneur from the picture of a market economy.[29]

But if we cannot even reason conclusively about the ERE, of what use can it be to our understanding of the world?[30] What we see here is, in fact, another instance of the failure of consequentialism. Stable conditions do not imply the eradication of profits. Entrepreneurial judgment is required to make them disappear. More generally stated, it is an impossible undertaking to define equilibrium in terms of conditions of action. There is no discernible final state of rest upon which the economy might more or less automatically "converge."

The ERE thus contradicts itself. However, even if, for the sake of argument, we accept such a self-contradictory equilibrium construct, we still encounter another problem. For the ERE is not in harmony with Mises's general view on the nature of economic science. He claimed that economics is composed of synthetic propositions *a priori*, that is, propositions that are derived from unaided reason *and* directly applicable to the real world. Now, one can hardly pretend that the ERE agrees with this conception. It obviously does not hold true wherever there is human action, and could never be realized in a world of human beings. It is simply an arbitrary and unrealistic assumption.

Mises did not integrate equilibrium into his general *a priori* approach. He found it an uncomfortable but necessary crutch, a tool of thought, an "imaginary construction." The ERE was the second-best solution while a genuinely economic approach to equilibrium was lacking. This begs the question of why Mises adopted the ERE rather than another view of equilibrium. The answer might be found in the following statement (Mises 1998, p. 291): "If we want to construct the image of changing economic conditions in which there are neither profits not losses, we must resort to an unrealizable assumption: perfect foresight of all future events on the part of all individuals."

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[29]Mises (1998, p. 249). For a somewhat different perspective on the inconsistencies of the ERE, see Cowen and Fink (1985). As Salerno (1993; 1995, p. 306) points out, in *Mises's* view, the validity of economic theorems does however not depend on the possibility of reaching the ERE.

[30]As Cowen and Fink (1985, p. 868) point out in a related criticism, "if the ERE reflects everything that the world is not, introducing a change into the ERE and letting it work its way through the system cannot be a promising endeavor. At best, all such a procedure could be used for is to tell us how the real world does not react to change."

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However, as we have seen, a sound equilibrium concept does not presuppose that individuals *know* the future, only that they *can* act successfully under any circumstances. This cannot be denied without contradiction. Moreover, equilibrium analysis does not presuppose an "image" of an economy in equilibrium throughout a given period. It refers to concrete individual actions, without regard to the rest of the economy. The possibility of successful action is a real feature of any human world, whereas the assumption of evenly rotating robots (of which the ERE is composed) is admittedly unrealistic.

Hence, the decisive consideration is that there *is* a genuinely economic approach to equilibrium. One does not need the ERE to analyze profits and losses. Basing equilibrium analysis on the categorical distinction between success and failure is in perfect harmony with Mises's view on economic science. In the approach outlined above, equilibrium is part of a comparison of two possible outcomes of action. Success and failure both refer to *human action*, and cover its entire range. Their distinction is both *a priori* and directly applicable to our understanding of any instance of human action in the real world.

On Hicks's Concept of Counterfactual Equilibrium

In one of his last works, Sir John R. Hicks defined equilibrium as "a condition in which all actors were taking all opportunities for gain that were open to them."[31] With this notion of such universally successful action at hand, characterized the "equilibrium method" as follows:

> A model of this kind [in which all market participants act under correct expectations] is not realistic; it makes no claim to be realistic. We are just to use it as a standard of comparison with the actual. For the historical application, at least, it is not inappropriate. We admit that in actuality, in "1975," things that were unexpected did happen, so that there was no such equilibrium during that year. But the model is to show us what *would have happened* if some cause had been different. . . . So the model can be, indeed should be, in equilibrium; though reality is not. (1979, p. 83)

Hicks thus clearly perceived that equilibrium analysis is counterfactual in nature. In his eyes, equilibrium analysis does not contrast a living and a dead world, but two living worlds.

This view is, however, defective in several important respects. First, in Hicks's eyes, equilibrium is by and large a (counterfactual) yardstick by which we evaluate the real world. He did not understand that equilibrium is part of a

[31]Hicks (1979, p. 78). Note that, according to this definition, equilibrium can prevail under *any* circumstances (see also p. 46).

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larger whole—namely, equilibrium analysis—or that the latter is a method, as opposed to a mere standard of comparison. Not surprisingly, counterfactual analysis is for Hicks a one-sided affair. The ideal is ever the unrealized model of the theoretician. It *cannot* be realized on earth and identified as such by comparisons with inferior counterfactual alternatives.

Second, it is not entirely clear to what extent Hicks fell prey to the consequentialist fallacy. His statement, that the economic model's function is to show us what would have happened if "some cause" had been different obtains only if by "cause" he means human choice, and nothing but human choice.

Third, Hicks did not understand that the counterfactual comparison must be founded on *individual* actions. His view is of the year 1975 *as a whole*. Individual equilibria are impossible in this approach; in ours, they are not.

Fourth, as an offspring of this holistic approach, Hicks does not compare the implications of (successful as opposed to erroneous) *choices*. He compares the effects of expected *events* to the effects of unexpected events. At first glance, this might seem to be only an insignificant difference that refers to the point of view from which the comparison is performed. However, a closer look reveals that Hicks's approach makes it almost impossible to apply equilibrium analysis. For when analyzing a disequilibrium of the entire economy, it is not clear at all *which* cause would have had to be different to bring about equilibrium. Any *single* event that would have been different not only would have rendered some actions successful, but it would also have rendered other actions unsuccessful. Therefore, it would be impossible to discern something like the "overall" effect of a single event on the fulfillment of expectations. Only a simultaneous consideration of *all* events could lend significance to an "equilibrium of events" as a standard of comparison. Yet, even then, the counterfactual variation of a single event would be meaningless since it could be in equilibrium only with reference to all other factors.

As a consequence, this approach would be completely worthless in practice. Even if it were possible to construct equilibrium by referring to events instead of individual choice (which is not the case), one would end up comparing one totality of ideal events to another totality of real events only to state that they are different in almost every respect. What kind of "analysis" would this be? The general conclusion to be drawn from these considerations is that it is not only extremely difficult to define equilibrium by referring to conditions of action, but is an entirely worthless undertaking: it does not advance our understanding at all. By contrast, focusing our analysis on human choice, we arrive at clear-cut distinctions that, as we will see, apply to both historical analyses and political decision making.

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On Selgin's Concept of Subjective Profit

Among the noncomparative approaches to equilibrium analysis, one can distinguish three primary groups.

The first is composed of those economists who claim that the economy is always in equilibrium. We addressed this group in our critique of consequentialist approaches that attempt to explain why and when equilibrium exists.

According to the second group, in which we find the ultra-subjectivists, applications of equilibrium analysis presuppose that acting man has knowledge of the future. Because this is not and can never be the case, equilibrium economics simply cannot help us understand the real world. We will deal with this view in the next section.

The third group attempts to establish a kind of middle ground between the two previous positions. These economists want to conserve the sound tenets of equilibrium analysis without, however, abandoning the notion of profit and loss, and without falling prey to ultra-subjectivist nihilism. Basically, there are two solutions within this middle-ground group. The first is the Hayek–Kirzner theory of the market process, an outstanding example of consequentialist reasoning (for a critique, see Hülsmann 1997).

George Selgin proposed the second solution in his important essay *Praxeology and Understanding*. According to his approach, profit and loss is an entirely subjective phenomenon that defies objective analysis. Selgin (1990, pp. 40–41) explains that:

> entrepreneurial profit opportunities in general are ephemeral phenomena, formed in the imaginations of enterprising people and *defined* by the very actions that "eliminate" them.
>
> It follows that praxeology must refrain from grouping the services of enterprising people according to "objective" standards, referring to earnings differentials as entrepreneurial profit. It instead assigns these differentials to the category "rent to labor services." Such rent may be said to include an element of profit only insofar as it actually gives rise to imitation by other individuals or to replication by the entrepreneur in question. . . . Every entrepreneurial action . . . begins with the subjective imagination of a profit opportunity (or belief that a loss may be avoided) and ends with the destruction of the imagined opportunity. This . . . is what praxeology means when it asserts that all action is "equilibrating," i.e., that action leads to the systematic elimination of profit and loss.

Let us begin our assessment with two minor remarks. First, the expression "profit gives rise to imitation or replication" seems to imply some sort of

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consequentialist reasoning. But what Selgin delivers, in fact, is an essentialist definition of profit and loss. He clearly states that profit or loss is given only "*insofar* as it gives rise."

Second, Selgin's comes close to a contradictory *regressus ad infinitum*. For although profit and loss are subjective, this subjectivity refers to the interpretation of monetary surpluses. It is therefore in the earning differential of a *past* action that an entrepreneur sees a profit opportunity, which in turn "gives rise" to imitate or replicate this action. However, this past action must have been performed in light of a preceding interpretation, which must have referred to earlier earning differentials, and so on. There is a regress on evermore past actions and earning differentials. Clearly, at some point in the past, somebody must have initiated production for the market. There must be a logical stopping point for this regress. In Selgin's account of it, however, there is not. The first entrepreneurs cannot rely on past earning differentials because they are the very ones who had brought about the first earning differentials. However, this is not a very grave objection, because the important aspect of Selgin's definition is that profit and loss are *subjective*. They are emanations of the individual's arbitrariness and therefore do not require a foregoing interpretation of monetary surpluses.

The decisive shortcoming of Selgin's approach is its very subjectivity. What Selgin has done, in fact, is to define away both error and disequilibrium. In light of his definition, there *can* be no failure. Error is caught up in the subjective box of imagination. It is "ephemeral"; it peeks through only at the very instant of choice, and then only in the mind of the decision maker. But it cannot manifest itself in *ex post* reality. On the premises of Selgin's approach there can be nothing on the unhampered market but equilibrium.

As far as definitions are concerned, this procedure is unobjectionable. However, it is one thing to propose a definition and another to make it stick. Choice exists. Choice *does* imply the possibility of error. Erroneous action *has* real-world repercussions. Not only is it impossible to deny these facts, but everybody recognizes them daily. People look back on what they have done, or have not done, and say to themselves, "I should have done this rather than that." This goes unnoticed in Selgin's approach. As he has it, people think of other actions just before they make their choice. Whatever happens afterward is just reward for their labor services. This blurs the important difference between profit and loss as choice-dependent income (which can be arbitraged away), and income for the specific qualities of one's labor services (which cannot be arbitraged away).

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And even if one were willing to cling to Selgin's definition, one could not fail to notice that it does not even begin to consider choice as possible error.[32] Attempting to fill the gap in equilibrium theory, Selgin only creates another void when trying to distinguish between the various forms of market income.

ON THE ULTRASUBJECTIVIST REJECTION OF EQUILIBRIUM ANALYSIS

Let us now deal with the subjectivist rejection of equilibrium economics. According to Shackle and Lachmann, the champions of ultrasubjectivism, equilibrium economics adds nothing valuable to our science. Their criticisms focus on the idea that equilibrium implies the absence of uncertainty. They believe that equilibrium analyses are useful only if equilibrium's existence can be proven, or at least appear possible to prove. Yet they also emphasize that no theory can explain why equilibrium exists or is even likely to exist in reality. From this correct observation they conclude that equilibrium is useless for economic analyses and, moreover, that economic science cannot be used for predictions.[33]

Though, it *is* possible, to conceive of equilibrium without renouncing uncertainty, and such a construct certainly adds something to our knowledge of the real world. The very existence of the approach we have outlined in this essay refutes the far-reaching claims of the advocates of ultra-subjectivism. And even if one denied the validity of this approach, one could not subscribe to their tenets. Even if a conception of equilibrium without uncertainty could not add to our understanding of real human action, it does not follow that there *can* be no realistic equilibrium economics. Shackle and Lachmann did not even attempt to explain why realistic equilibrium analysis is impossible from the outset. Yet it is precisely such an *a priori* proof that is required to justify their general claim. All they have done is point out the insufficiency of *past* approaches to equilibrium analysis.

This leads us to a concluding general interrogation about the significance of subjectivism to economic science. Is subjectivism one of its central features? To some degree, at least, the opinion that this is the case seems to rest on semantic vagueness. Indeed, the term "subjective" has two quite different meanings: (a) "arbitrary" and (b) "individual."

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[32]For a critique of the subjectivist denial of error, see Kirzner (1992, pp. 23ff.).

[33]See, in particular, Shackle (1972) and Lachmann (1994). Similar criticism is to be found in Kaldor (1972). A forerunner is Struve (1936, p. 522f.), who also mentions Simiand (1932, p. 93).

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There can be no doubt that modern economics is a subjectivist science in the sense that it deals with *individual* actions. Starting with the writings of Menger, Jevons, and Walras, economists abandoned the *class* analysis of their predecessors. There were no longer capitalists, landlords, and workers buying and selling labor, land, capital, and consumer goods. But individuals buying and selling specific quantities and qualities of goods. With the help of the new marginal analysis it was possible to demonstrate for the first time that, ultimately, all prices paid on the market, as well as the structure of production, could be explained in terms of individual utility. However, utility was not conceived to be subjective at all. It was not a matter of individual arbitrariness, not something determined by choice. Neither Menger, Walras, nor Jevons believed that subjective decisions were the standard by reference to which prices could be explained.[34]

The new perspective was first and foremost the achievement of an analysis cast in terms of individual actions and specific quantities of goods. By contrast, subjectivism in the sense of arbitrary decision making or freedom of choice did not play a comparable role, at least in the initial phase of the marginal revolution.[35]

It was only later that economists became increasingly aware of the problems inherent in a "logic of choice." How can one even assume that choice is both free *and* subject to laws without running into inner contradictions? For economists working within the framework of the cost-of-production theory of value – that is, within the framework of Smith and Ricardo – this problem did not and could not exist. As they saw it, prices were objectively determined by toil and trouble, that is, by the cost of labor. Individual decisions did not interfere here at all. Obviously, what one wants to invent is irrelevant for the question of how much labor one *must* invest in order to produce a given commodity. Preference rankings, interpretations, and anticipations were also irrelevant in this regard. Choice could not determine value and prices.

[34]See Menger (1871, p. 121f.) and Walras (1988, §50), in particular the passage "ces dispositions n'en existent pas moins." Jevons (1957, p. 38) advocates Say's definition of utility, as, the "faculté qu'ont les choses de pouvoir servir à l'homme." Jevons expressly refers to the distinction established Condillac established between utility and value. See ibid., preface to the second edition, p. XXVIII; Condillac (1795, p. 6). Characteristically, one of the first historians of the Austrian School, James Bonar (1996, p. 12), described the theory of value in Menger's *Grundsätze* as an "investigation of certain principles, fixed independently of individual will, which determine what makes a thing 'useful,' a 'good,' and a thing 'valuable' to me." See also Bonar (pp. 13, 26). Incidentally, Karl Pribram (1983, p. 612) argues that even the Scholastics had an equilibrium concept in the form of equivalence between actual value and intrinsic value, and Peter Struve (1936, p. 486) observes the notion of equilibrium price in Aristotle's *Nicomachean Ethics*.

[35]See, for example, the categorical statement in Rosenstein-Rodan (1927, p. 1210).

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Compare this to the viewpoint of modern economists, who try to deduce the analysis of prices and the structure of production from the analysis of individual utility. The minor problem is to discern the objective character of utility. The utility of consumers' goods is as objective as the physical productivity of a machine. Neither can be discussed away by acting man. The major problem is inherent in the fact that all our actions *are* manifestations of choice. We know, to be sure, that neither the *rightfulness* nor the economic *success* of human action depends on human will. Yet we cannot dispute the fact that our actions *per se* are exclusively directed by subjective interpretations of our environment. This being so, how can one reconcile the notion that there are laws of human action, laws of market pricing in particular, with the existence and nature of choice? This is the fundamental problem of modern economics, and the purpose of this article has been to show how it could be solved in the case of equilibrium analysis.

CONCLUSION

Science seeks to explain objective facts by reference to constant relationships that link them to other facts. Yet economic science deals with human action, which is directed by individual choice, which seems by its very nature to contradict the notion of constancy. How can we reconcile the idea that there are laws of human action, that manifest themselves in market prices and the structure of production, with the idea that there is also freedom of choice? All modern discussions of the relevance of equilibrium economics revolve around this problem.

We have argued that there are constant relationships *in* choice itself—in particular, in the dichotomy of success and failure. Recognizing this fact paves the way toward a realistic equilibrium analysis, which consists in comparing an actual choice with its counterfactual alternatives in terms of success and failure. This approach underscores Mises's insight that equilibrium analysis deals only with a very limited range of phenomena—it is *only a part* of economic science. And, in distinct contrast to all previous approaches, it does not rely on fictions of the mind or unrealistic constructs. It is an integral part of a realistic science, a precious tool for the understanding of reality.

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ARE MMMFS MONEY?

JEFF HAYMOND

Two decades have passed since the Federal Reserve instituted the "monetarist" experiment. This attempt to implement Fed policy based on quantity theory prescriptions was flawed almost from the beginning,[1] with one monetary aggregate after another being tried and eventually discarded. The Lucas critique was in effect, and, with changing technological innovation, exactly what was "money" became more and more difficult to define. The Fed thereafter resumed targeting the Fed Funds Rate and de-emphasized monetary aggregates. Monetarism as a theory was widely discredited, at least in a practical sense for public policy.

Changing monetary instruments and institutions may affect other theories as well. The central core of the Austrian business cycle theory (ABCT) suggests that monetary expansion is responsible for the boom-and-bust business cycle (Mises 1981, pp. 403–04). But what measure do we use to determine whether we have a monetary expansion? Differing Austrian examinations of business cycles have used slightly different definitions of money, for each investigation,[2] but central to each is the role of fiduciary media, which traditionally are expanded when the central bank increases reserves. Previous investigations have limited the fiduciary media in question to checkable deposits (and notes when legal for banks to issue). But as institutions change and financial

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[1]Milton Friedman claimed that the policy followed was anything but monetarist, as monthly money supply figures were highly variable—exactly opposite of a monetarist approach. The Fed, for its part, acknowledged the lack of precision in hitting targets, but claimed it was doing its best, given changing money demand functions.

[2]Rothbard was widely criticized (wrongly, in my view) for including the cash surrender value of life insurance policies in his definition of money in *America's Great Depression*. Recently, Hughes (1997) simply used M2 in his analysis.

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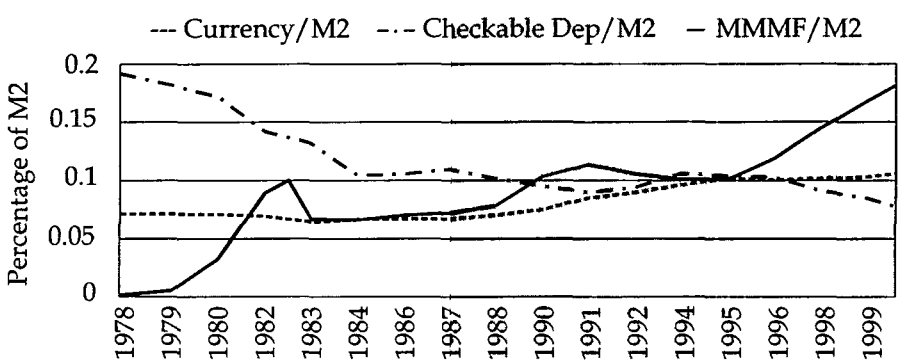
innovation increases, is this the only form of fiduciary media that Austrians should consider in their views on business cycles? This article will specifically examine the question of whether money market mutual funds (MMMFs) are fiduciary media in a Misesian sense. If MMMFs are considered money, for instance, who creates them? Are there any direct limitations on their creation via Federal Reserve action? If not, how are these monies affected by Fed policy? And if MMMFs are money, are there nonbank sources of inflationary credit that can initiate a business cycle? This article will argue that, in the economically relevant way, MMMFs *are* money, and while nonbanks cannot *initiate* a credit expansion, their ability to expand their balance sheets should be a relevant factor for Austrian analysis.

For Austrians as well as most economists, there is little debate as to the "moneyness" of fiat currency or checkable deposits; yet the question of MMMFs offers the possibility of a fruitful debate. Consequently, while this article will not assess whether each particular asset is money, the principles developed will apply when assessing any monetary instrument. The other main reason for focusing on MMMFs, and the nonbank intermediaries that issue them is simply the tremendous increase in the number of MMMFs, in the 1990s. This is contrary to the approach that White (1989, p. 207) took: he examined traveler's checks in-depth, even despite their lack of practical importance to the money supply (to illustrate the principles involved). Conversely, we will look at MMMFs precisely because of their growing importance to the broader money supplies. Figure 1 below shows the growth of MMMFs as a percentage of the broader money supply M2, contrasted with a reduction of checkable deposits as a percentage of M2.

Figure 1
Percentage of M2



Source: Federal Reserve

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WHAT ARE MONEY MARKET MUTUAL FUNDS?

While most readers are familiar with MMMFs, it may be useful to review what they are and how they operate. An MMMF is an equity fund that typically purchases highly liquid, high-quality, short-term debt instruments such as T-Bills or commercial paper. A purchaser will transfer money balances to the fund, which will purchase shares of the existing portfolio with a nominal par value of one dollar. If the value of the portfolio changes, the number of shares within an individual's account will change to maintain the par value per share. MMMFs do not have any established reserve requirements, but typically hold a small amount on deposit with banks in the form of checkable deposits for adverse clearings. Since they hold highly liquid assets, if their outflows exceed their inflows, they can easily sell assets to replenish their cash reserves with low transaction costs. In fact, MMMFs must sell assets in exchange for demand deposits in this case, since that form is required by the ultimate recipient of an MMMF check.[3] Most MMMFs issue checkbooks and allow withdrawals, although they typically must exceed a certain dollar amount. Purchasers sacrifice some liquidity (they can write checks only above a certain amount), but have continuous access to their money. Checks drawn against MMMFs are generally accepted anywhere demand deposit checks are accepted. Subjectively, this asset appears to be a present good to those participants. Although the MMMFs are not debt claims, but equity claims, the possibility of failure to redeem is extremely remote, so much so that in practice MMMFs are usually seen simply as another form of cash.[4] MMMFs are an increasing part of individuals' portfolios, while reducing the requirement for narrower versions of money. MMMFs are not insured by the federal government, however, but most are insured by the Securities Investor Protection Corporation (SIPC) up to $500,000 per customer, but this does not ensure redeemability at par.[5]

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[3]I thank an anonymous referee for pointing out the need to be more clear in this matter. It is certainly true that the assets held in the MMMF are not money directly, but must be converted into a narrower form in some market. The crucial point, however, is that this conversion is virtually invisible to the market participants who write checks against their MMMF accounts, as well as to those who accept them. This also legitimately implies that, according to the same logic, as the referee points out—other funds that issue check-writing privileges could be considered "money." I would agree when and if market participants subjectively assess them as they do MMMFs—but, given their less certain redemption at par and low relative usage, this does not seem germane just yet.

[4]To my knowledge, there has only been one failure of an MMMF, and it paid something over ninety cents to the dollar.

[5]See http://www.sipc.org/faq/content/faq1.shtml, however it explicitly states "An investment in mutual fund shares, including money market mutual fund shares, is not protected or guaranteed by SIPC against loss in value."

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ARE MONEY MARKET MUTUAL FUNDS MONEY?

This issue has recently become a point of debate between two financial market participants[6] who both claim to analyze money from an Austrian perspective. Frank Shostak (former chief economist for Ord Minnett), claims that MMMFs are not money, for to create shares in an MMMF, someone must first surrender real money (2000a). Accordingly, creation of MMMFs is a credit transaction, not a money creation. Doug Noland (financial market strategist for David Tice and Associates) asserts just the opposite—that growth in MMMFs is the outward manifestation of a dangerous credit bubble. He argues that MMMFs possess the widow's cruse, and can infinitely multiply deposits as the financial system requires. (While outside the traditional academic mainstream, we should seriously consider market participants' claims since they put more on the line than their academic reputations.)

WHY THE CONCERN OVER MONEY?

While seemingly obvious, the answer to this question will help us understand which assets should be considered money. Central to our review will be an application to MMMFs of concepts from Mises's *The Theory of Money and Credit*. Mises addressed this question as follows:

> In attempting to draw a line of division between money and those objects that outwardly resemble it, we only need to bear in mind the goal of our investigation. The present discussion aims at tracing the laws that determine the exchange ratio between money and other economic goods. This and nothing else is the task of the economic theory of money. (1981, p. 65)

We therefore ask what is money because of the way certain assets and claims influence its price relative to all other goods. As will be discussed below, Mises shows how some assets that are not money proper but substitutes for it can influence the price level. These same money substitutes, while disturbing the objective exchange value of money, also disturb relative prices of nonmonetary goods as the introduction of new "money" necessarily affects the prices of some goods prior to others. These relative price disturbances send false signals to economic actors concerning consumer preferences, leading to malinvestments. As the introduction of the new money works its way through the economy, all prices will readjust to reflect true consumer preferences, revealing the bad investments—the boom is thus followed by a bust. This boom-and-bust business cycle caused by monetary policy is the second reason for being

[6]See Noland (2000b). Many of his commentaries for the first quarter 2000 deal with this subject, and are archived at www.prudentbear.com.

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concerned with money—the inflation we were originally concerned with is also a source of economic instability.

While acknowledging the technical and legal requirements for money, the important consideration for Mises was the actual economic effect of an instrument. As he notes, "it is not the formal technical aspect of a transaction but its economic character that determines its significance for us" (1981, p. 303). Further, "It is a mistake to deal with economic problems according to legal criteria" (p. 73). He also stresses that individual subjective valuations will determine the moneyness of an object—an individual treating and accepting an object as money carries more weight than some technical justification.

> [I]t is not the dead letter of the law so much as actual business practice that counts, so that some things function as fiduciary media, although they cannot be regarded as promises to pay money from the juristic point of view, because they nevertheless are in fact honored as such by somebody or the other. (1981, p. 311)

While disagreeing with this paper's conclusion regarding MMMFs, Salerno, Rothbard, and White share the belief that an asset's function determines its moneyness. Salerno applies this logic to savings deposits:

> The objection that claims on dollars held in savings deposits typically do not circulate in exchange . . . while not unimportant for some purposes of analysis, is here beside the point. The essential, economic point is that some or all of the dollars accumulated in, e.g., passbook savings accounts are effectively withdrawable on demand by depositors in the form of spendable cash. (1987, p. 3)

Salerno credits this "commonsense" approach to Melchior Palyi, who stated "In their own minds, money is what people consider as purchasing power, available at once or shortly. People's 'liquidity' status and financial disposition are not affected by juristic subtleties and technicalities."[7] Rothbard uses this approach to illustrate the moneyness of checkable deposits.

> It is important to recognize that demand deposits are not automatically part of the money supply by virtue of their very existence; they continue as equivalent to money only so long as the subjective estimates of the sellers of goods on the market *think* that they are so equivalent and accept them as such in exchange. (1978, p. 145)

Rothbard also says that it is not a question of whether an asset can actually deliver on its promises (i.e., fractional reserve banking ensures that not all

[7]Quoted in Salerno (1987, p. 3).

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claims for cash can be simultaneously satisfied), but the issue depends on the subjective estimates of the agents.

> Deposits are not *in fact* all redeemable in cash in a system of fractional reserve banking; but so long as individuals on the market *think* that they are so redeemable, they continue to function as part of the money supply. (1978, p. 145)

White also endorses this view. He (1989, p. 203) states, "From a subjectivist perspective, it is clear that the defining set of attributes of money is to be sought in the role that money plays (and alone plays) in the plans of individual economic agents." Salerno's "commonsense" approach will often be applied in subsequent sections as we argue that, in fact, MMMFs act as money in today's economy—both from an inflationary and a malinvestment perspective.

ARGUMENTS AGAINST MMMFS BEING MONEY

The debate over what constitutes money is not new, and there are probably as many different opinions as there are economists. Many of the important arguments over economic policy have arisen from disagreements over money—in one case, the Currency School was rejected because of its failure to understand that checkable deposits were as much money as banknotes (Mises 1981, pp. 407–11). In some cases, the debate is over no opinion: Alan Greenspan recently suggested that the Fed does not know what money is, so its control thereof is impossible.[8] In contrast, Rothbard, Salerno, and White have addressed this question from an Austrian perspective, offering methodology to characterize money.[9] Yet all are in agreement that MMMFs are *not* money. As White notes (and Salerno references in agreement),

> the item that the check-writing MMMF customer relinquishes (ownership of shares in a portfolio of assets) is not what the payee accepts (ownership of an inside-money claim to bank reserves). Because the actual MMMF shares are not what the second party accepts (or intends to accept), MMMF shares cannot be considered a generally accepted medium of exchange; hence they are not money. (1989, p. 213)

The first objection, therefore, is,

1. Technical considerations dictate that MMMFs cannot be money. One cannot call an asset generally accepted if what one party accepts is not what the other party relinquishes.

[8]Greenspan made this point during his Humphrey–Hawkins testimony of February 2000, when Congressman Ron Paul challenged him on the need to restrict money growth. I am unable to document this directly, although Shostak discusses it in "The System of the Money Supply" (2000b).

[9]See Rothbard (1978, pp. 143–56), Salerno (1987, pp. 1–6), and White (1989, pp. 203–14).

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Is this true? And, if it is, does this technical consideration overcome the practical functionality of MMMFs as an economic instrument? The previous quotations from Austrians, *including* White, suggest just the opposite. It is true that what one person offers in exchange is not what the other party wishes to receive. But if an individual accepts a draft on a MMMF and deposits it with his bank, which credits his account after it clears, this process is *exactly* the same as that for a checkable deposit. For economic purposes, they are identical. If there were differences in probability of payoff, a businesswoman might cast a more suspicious eye toward an MMMF draft. But, given their record, this is not a practical consideration.

When Osborne reviewed the question of what money is, he came to a narrow conclusion: only the monetary base can be considered money (1985, pp. 1–15). He founded this claim on three propositions: first, that money must enjoy routine circulation (i.e., be the medium of exchange). Second, that there is simultaneity of payments—"money is that, the entire stock of which can be spent at once." Third, that there be finality of payment: "money is that which pays for goods or services without creating a further debt for the payer" (p. 1). This may be an appropriate methodology when narrowly defining money, but even Osborne doubts it. He points out his own regrettable implications as follows,

> Our present analysis forces us to identify private bank notes as money even though they are economically indistinguishable from intangible balances. This unacceptable result proves the incompleteness of our definition of money. (1985, p. 14)

Osborne's definition raises a second objection where MMMFs are concerned.

2. Only the narrow monetary base meets the Yeager–Shackle requirements of routine circulation, simultaneity, and finality.

For our purposes, though, this addresses only half the problem. Money is of interest precisely because of its potential mischief in exchange (by its changing objective exchange value because of inflation or deflation) and possible business-cycle implications. But these considerations are not unique to narrow money; one must also consider the state of money substitutes in an economy—especially fiduciary media, which Mises refers to as circulation credit. Mises introduced the ABCT by noting the ability of fiduciary media expansion to initiate monetary disequilibrium. Thus, even if the objection is correct, it is inapplicable to traditional Austrian concerns over money.

Shostak offers a different critique of MMMFs as money, he makes the distinction between MMMFs as credit transactions and as credit creation. As he states,

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> Let's say Joe deposits $1 million with a money-market mutual fund. A mortgage supplier borrows the $1 million from the mutual fund and lends it to Jane, who in turn uses the $1 million to buy a house. The recipient of the $1 million, i.e., the seller of the house, deposits the money with a money-market mutual fund.
>
> Has the economy's money supply increased by $1 million? Not at all. What we have here is a credit transaction, money transferred from a lender to a borrower. Credit always involves the purchase of a future good by the creditor in exchange for a present good.
>
> Thus when Joe makes a loan to Jane, he temporarily transfers his ownership over the money. Similarly, when Joe invests in a stock or a money-market mutual fund, he gives permission to these institutions to invest his money. He has temporarily transferred the ownership of money to these institutions.
>
> Since a credit transaction is a transfer of saved funds from a lender to a borrower it does not result in the creation of new money, but simply new credit. This credit, however, is not harmful, for it is fully backed by saved money. (Shostak 2000a, p. 1)

3. A third objection, then, runs as follows: the purchase of MMMFs constitutes a credit transaction, not credit creation.

But in Shostak's example, has Joe really given up control of his money? If so, has he agreed to give it up for the period of time for which it is loaned out? Certainly not. If Joe sees an asset he wants to purchase, say a new car, he will not be the least concerned with whether his original money is loaned out somewhere else: he will simply open his MMMF checkbook and write the check. Were this liquidity not available, he likely would never have deposited his money with an MMMF. Once again, technical considerations do not outweigh the practical use of this instrument. Shostak would be correct *if Joe were truly surrendering control over this present good.* But he has no such intent. If he did, he would purchase the mortgage directly and obtain the higher yield. He is willing to accept the lower yield for the right to enjoy ownership of what he correctly considers a present good. In addition, this discussion doesn't address the possibility of MMMF multiple deposit expansion, which is reviewed below.

4. MMMFs are equity claims, not debt, so they can't represent any given amount.

Once again, this is a technical consideration (discussed more thoroughly below), which, while true, does not have practical significance. If, historically, there were more instances of MMMF failures that resulted in losses, this would be more relevant. But, given the low failure rate, it seems reasonable for people to believe that the shares they redeem will be at par.

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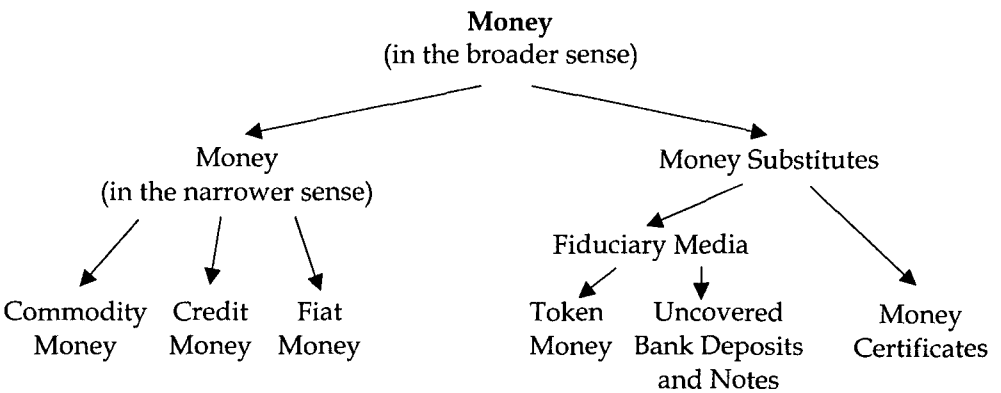
WHY MMMFS SHOULD BE CONSIDERED MONEY

In Ludwig von Mises's *The Theory of Money and Credit*, Appendix B offered the following taxonomy to describe money.



Clearly, MMMFs do not fall under the category of money in the narrower sense. But are they a type of fiduciary media? For Mises, money substitutes are "those objects that are employed like money in commerce but consist in perfectly secure and immediately convertible claims to money" (1981, p. 65). But as noted previously, these characteristics are subjectively evaluated. Consumers today seem to find a money-market fund as secure as a check, as do producers. No one could argue that they are perfectly secure, but one can make the case that they are as secure as checks, which are readily acknowledged as money.[10] Mises expands on this definition of perfectly safe, noting that "(money substitutes) are . . . absolutely safe as far as human foresight goes" (1981, p. 66). This seems a slightly less stringent requirement than "perfectly secure," and one that MMMFs have demonstrated throughout their history. They are also immediately convertible (subject to the minimum withdrawal amount) when used as a check. MMMFs thus seem to meet Mises's two requirements of money substitutes practically, if not juristically. As Mises again, states,

> A depositor of a sum of money who acquires in exchange for it a claim convertible into money at any time which will perform exactly the same service for him as the sum it refers to, has exchanged no present good for a future good. The claim that he has acquired by his deposit is also a present good for him. The depositing of the money in no way means that

[10]It is true that checks are guaranteed by the FDIC whereas MMMFs are not, but in today's environment of "too big to fail" and government bailout of any financial flop that threatens systemic crisis, is it reasonable to think the government would step in in the case of a large MMMF failure?

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> he has renounced immediate disposal over the utility that it commands. (1981, p. 301)

If this is true, what type of money substitutes are MMMFs? They seem to fall under the category of fiduciary media, money substitutes not fully backed by money for redemption purposes (p. 155). Clearly MMMFs are not fully backed, and, in fact, lack reserve requirements though they do keep certain reserves available to meet redemption requirements.[11]

As noted above, Mises considered the objective exchange value of money to be the central economic problem of money. To what extent—if any—do MMMFs change the objective value of money? For Mises (1963, p. 411), this is determined by the interaction of supply and demand for money, broadly defined (i.e., including money substitutes). MMMFs affect this money relation in two ways: they reduce the demand for narrow money (currency and demand deposits, as reflected in Figure 1) and they increase the supply of broad money. Both of these factors tend to reduce the objective exchange value of money, *ceteris paribus*. As Mises states,

> The cash balance held by an individual need by no means consist entirely of money. If secure claims to money, payable on demand, are employed commercially as substitutes for money, being tendered and accepted in place of money, then individuals' stores of money can be entirely or partly replaced by a corresponding store of these substitutes. (1981, p. 154)

As argued above, all of these features are subjectively inherent to MMMFs, which allows a reduction of narrow money balances.[12]

MMMF MULTIPLICATION

According to Austrian theory the ability of banks to issue fiduciary media has been the source of both inflation and business cycles. Banks create loans on the asset side of their balance sheets while adding checkable deposits on the liability side. The checkable deposits represent a claim on, without the saving of, present goods, based on a promise to pay back future goods. Without new present goods, the issue of further claims against present goods is necessarily

[11]This amount obviously depends on each institution, but some sources estimate that MMMFs keep roughly one-half of one percent available for redemption.

[12]As an anonymous referee points out, for the previous analysis to be true, MMMFs cannot be perfect substitutes; otherwise, MMMFs could only work through the supply side, not the demand side. While perhaps not as clear as I would like, the reference to Mises's distinctions between broad and narrow money is an attempt to show that, despite the fact that MMMFs and narrow money are imperfect substitutes, MMMFs nonetheless can influence the critical monetary question—the exchange value between money and other economic goods. I in no way want to suggest that they are perfect substitutes (especially since the act of choice illustrates the fact that there can be no such thing as "perfect" substitutes).

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inflationary. And, according to Austrian theory, the increase in fiduciary media lowers the market rate of interest below the natural rate of interest, giving entrepreneurs false incentives to invest in higher order goods, which lead to malinvestment. The initial boom (due to the increase in fiduciary media) will inevitably lead to a bust, for increased investment cannot be sustained in the absence of a change in consumer preferences. The process of money creation can be found in any macroeconomics or money and banking textbook. An individual bank could not expand the money supply independent of the expansionary actions of other banks: as one bank issued checkable deposits at a rate greater than that of its competitors, it would suffer adverse clearings, lose reserves, and be forced to pull back on its expansionary efforts. If, however, all banks were issuing new fiduciary media at the same high rate, no adverse clearings would take place.[13] In the face of reserve requirements, money multiplication could take place up to the inverse of the reserve requirements. In actual practice, money multiplication is much less than this, as "leakage" occurs. This leakage can occur as either the bank decides to maintain excess reserves or as the public demands additional currency. But if MMMFs are money, how do they fit into this multiplication scheme?

MMMFs have no statutory reserve requirements, so, theoretically, they could expand their balance sheets infinitely. Let's review one possible scenario. If Bill deposits one million dollars with Vanguard money market mutual fund, Vanguard can purchase one million dollars of GE's commercial paper. GE can spend this money on anything it wants, but let's say they use it to finance car loans. If all car dealers deposit this money into their own MMMFs (say Fidelity MMMF), the process can continue. Fidelity then uses the one million dollars to purchase mortgage-backed securities from Fannie Mae, who then uses the sum to purchase more mortgages. This money will go ultimately to sellers of home equity (either direct sales or refinances). If these individuals deposit it directly into Schwab's MMMF, then more commercial paper is bought, and more spending can occur throughout the economy. Theoretically, this could lead to infinite expansion—if no *leakage* occurs. Leakage occurs when the recipient of newly created credit demands a form of money, narrower than that offered. In the case of MMMFs, the chance of leakage seems greater than for checkable deposits; they could leak into demands not only for currency or excess reserves, but for checkable deposits themselves. The whole point of MMMFs is their liquidity, coupled with higher interest rates than

[13] As Selgin (1988) points out, however, when gross clearings increase, the probability of insufficient reserves increases, thus providing a natural limit to the expansionary capabilities of the whole system. This natural limitation is obviated with a central bank lender of last resort, thus greatly increasing the possible inflationary expansion.

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those of checking accounts. Clearly, many consumers are going to redeem them and the multiplication process will fall far short of infinite. In fact, as the redemption process occurs, multiple deposit contraction will take place, just as it does in the banking system when individuals demand cash from their checking accounts.

Nonetheless, the expansion of MMMF balance sheets is inflationary since more claims to present goods are created without any abstention of claims to present goods. Many commentators have argued that credit is only created by the Federal Reserve. This is not strictly true as the above discussion demonstrates. One should say, rather, that the Federal Reserve is responsible for the initiation of all credit booms. In our example above, Bill originally received one million dollars that was the result of the Fed's money creation process. But, once it is initiated, the Fed cannot control how this credit will multiply throughout the system, especially when there are no statutory reserve requirements. But this is really no different than what happens with credit creation via the banking system. The Fed supplies reserves, then the banking system and the public's decisions regarding excess reserves and currency requirements dictate the actual money created. It is true that, ultimately, one can lay the blame for excess credit creation at the Fed's feet, for they must supply all reserves (both for the banking system and MMMFs). But it is also true that the control of money is difficult enough (in a fractional reserve system) in the banking system (especially during the short run), and much more difficult with nonbank financial intermediaries (due to lack of reserve requirements).

As discussed earlier, some analysts like to separate MMMFs from the banking system because they are equity claims instead of debt claims. If everyone tried to redeem MMMFs for cash, MMMFs would be forced to liquidate their assets, ultimately leaving the holders with less than par value. This is the same situation that occurs for the banking industry without a lender of last resort, if holders of checkable deposits demand cash. Loans have to be liquidated at fire sale losses, and banks would have to declare bankruptcy. The only difference is that the MMMF would not have to declare bankruptcy, for holders of these funds are residual claimants. The banking system overcame this problem upon creation of the Federal Reserve and FDIC insurance, which the MMMF industry does not have. But, as White argues, countries with loosely regulated free banking did not experience the sort of banking panics seen in the U.S. (1999, pp. 69–73). This suggests that private markets can devise run-proof institutional arrangements. Cowen and Krozner argue that the residual claimant status itself makes MMMFs inherently run-proof (1990, pp. 225–28). As mentioned previously, this technical distinction isn't economically meaningful,

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as both holders of MMMFs and acceptors of MMMF checks treat them as virtually *identical* to checkable deposits. And, as most Austrians agree, the appropriate test is how an asset functions economically.

IMPLICATIONS AND FUTURE WORK

A traditional concern of many Austrians is the practice of fractional reserve banking.[14] They hold that fractional reserve banking as an institution is responsible for all inflations and the boom–bust cycle itself. The central bank is not the primary, but rather a secondary problem because it is only through central banks that fractional reserve banking, with the effective "cartelization" of private banks, can survive and flourish (Rothbard 1994, pp. 54–69). But if the analysis above is correct, even if we were to have a one-hundred-percent reserve gold standard, we could still have potential inflation and business cycles with creation of money substitutes such as MMMFs. As monetary innovation has occurred, fractional reserve banking is no longer the *only* potential source of mischief.[15] Implicit government guarantees are also responsible, for it especially considering the recent experience of "too big to fail." One has to wonder if people would be as willing to accept MMMFs as a money substitute if the government were not there to *resolve* every crisis and add to the moral hazard problem. Just as Garrison (1994) expanded ABCT to include risk transfer via FDIC guarantees, the moral hazard "bubble" created by the IMF and other agency bailouts may be another potential manifestation of ABCT. The willingness of individuals to hold less liquid forms of money may simply reflect this change.

The implications of this are clear. Austrians should be concerned about the growth of MMMFs. They can be inflationary, and Austrian theory provides a unique perspective to assess their growth. Austrians are virtually alone among current theorists in that they discuss the distributional aspects of inflation—money is not neutral, because it is created and injected in a certain place and time. Not all goods see the effects of inflation equally. When new money is created by banking loans, the items purchased with the newly created checkable deposits will see their prices rise first (Cantillon effects). As money for those goods is subsequently spent, the inflation will pass from good to good. Only in the long run will the inflation be seen throughout the system. But money is not neutral, even in the long run, as those who were able to spend it

[14]See for example, Rothbard 1994. This concern has been contrasted by other Austrians, such as Selgin and White 1996, who are more concerned with the institution of central banking itself.

[15]Given the reasons for much of the financial innovation (i.e., to avoid the effects of inflation), one naturally wonders how the course of innovation would have progressed absent fractional reserve banking.

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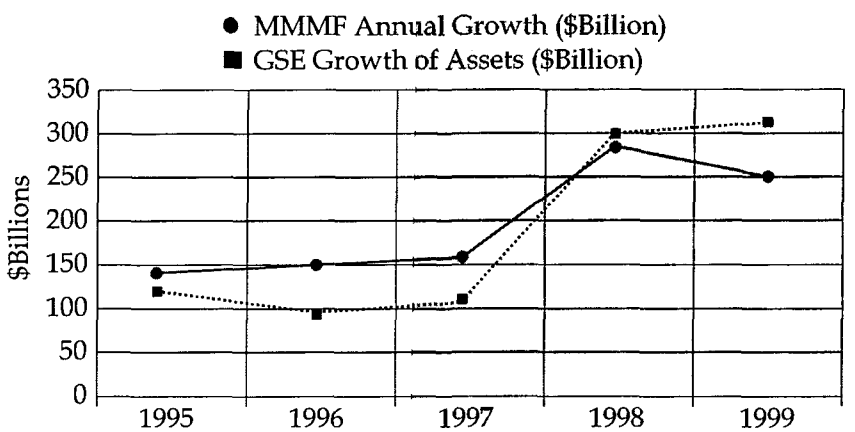
first benefited at the expense of those who received it last. In standard Austrian theory, the producer's goods purchased first will be the first to see the inflation, and only in the end will it spread throughout the consumer's goods.

With the Austrian perspective, one can ask the question: which prices will rise first as a result of the money creation process inherent to MMMFs? In the case of MMMFs, the money goes first to the sellers of the highly liquid debt instruments (usually commercial paper, mortgage-backed securities, and T-bills). Figure 2 below suggests that there may be a relationship between MMMFs and the explosive growth in the balance sheets of government sponsored-enterprises (GSEs), as Noland (2000a) suggests.

Unfortunately, there are no detailed aggregate statistics available on the overall composition of MMMF assets (when we aggregate as Hayek suggests, we lose the very granularity needed to assess), although individual MMMF balance sheets are available. But if MMMFs invest in mortgages and the commercial paper of Wall Street firms (they do, the only question is the degree), then Austrian theory suggests that growth in this form of fiduciary media would initially lead to inflated asset prices and a subsequent rise in consumer prices. This issue should be explored further.

In addition, if GSEs account for such a large part of the MMMF increase, what role does their implicit U.S. government guarantee play in the expansion of their balance sheets? Treasury Undersecretary Gensler recently noted that "The advantages of GSE status have also enabled the GSEs to grow rapidly and gain an increasing share of the capital markets. The GSEs now control a central position in the mortgage market and an increasing share of the U.S. debt

Figure 2
MMMF and GSE Growth



Federal Reserve Z1

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markets."[16] These advantages have led to their dominance in capital markets. The large increases over the last two years are clearly becoming a significant issue:

> The $1.4 trillion of GSE debt is large on any relative scale. It is now roughly the size of the entire municipal bond market—the outstanding debt of the fifty states and localities that issue publicly traded debt. The GSEs' debt of $1.4 trillion is now more than one-half of the $2.7 trillion of outstanding privately held marketable Treasury debt. Adding the $1.2 trillion in GSE-guaranteed mortgage-backed securities to the mix, GSE involvement in the credit market is approaching the size of the Treasury market. (Gensler 2000, p. 3)

ABCT depends on the government's central bank sending entrepreneurs false signals regarding the time preferences of consumers. Are GSEs, through their implicit government guarantees, able to expand their balance sheets in excess of what the market would do? The GSE status allows them less expensive capital as well. As Gensler notes, "Over the last six months, the GSEs borrowed at approximately 40 basis points less than AA-rated banking and financial firms on one- and five-year debt."[17] Does the lower cost of mortgages result in malinvestment and inflation in the housing market? These are all questions to consider once we determine that MMMFs are money.

CONCLUSION

In the last twenty years, continual financial innovation has led to the increased use of MMMFs as a substitute for checkable deposits. While many technical considerations suggest that it is inappropriate to list MMMFs as money, traditional Austrian thought has emphasized money's subjective aspects: money is what people think it is. Since the public increasingly uses MMMFs as money substitutes, they are money in all practical respects. Every technical consideration that would restrict the use of MMMFs as a type of money thus crashes against the rocks of practical, everyday experience.

While nonbank financial intermediaries can expand this type of money substitute, the Federal Reserve must still provide initial credit for any multiplication to take place. These nonbanks are even more difficult for the Fed to control, for they face no reserve requirements. But, on the other hand, there are more opportunities for leakage from the system, thereby limiting potential credit expansion. Nonetheless, this is an aspect of money that is becoming increasingly important. And if, as seems likely, MMMF expansion stimulates different

[16]See Gensler's (2000, p. 3) testimony to the House Banking Subcommittee on Capital Markets, Securities and Government Sponsored Enterprises.

[17]Ibid., p. 2.

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sectors of the economy than the banking system generally, we should expect to see monetary inflation manifest itself in different ways than previous inflations.

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THE MYSTERY OF THE MONEY SUPPLY DEFINITION

FRANK SHOSTAK

According to mainstream economics, the validity of various definitions of money can be ascertained by means of a statistical test. What determines whether money M1, M2, and the other Ms are valid definitions is how well they correlate with national income. Most economists hold that, since the early 1980s, correlations between various definitions of money and national income have broken down. The reason for this breakdown, it is held, is that financial deregulation has made the demand for money unstable. In short, the nature of financial markets has changed; consequently, past definitions of money no longer hold.

WHY THE MAINSTREAM APPROACH IS FLAWED

Observe that, for the mainstream, the definition of money is established through an arbitrary mixing of various liquid assets and then correlating this mixture with another dubious statistic labeled national income. In other words, any mixture of liquid assets will be classified as money as long as this mixture passes the correlation test. Now, if any mixture of liquidity is accepted, why not include retail good inventories? After all, these inventories might be as liquid as stocks or bonds. Yet, no one would consider these inventories as part of the money supply (Rothbard 1978, p. 149).

But no definition can be established by means of a correlation. The purpose of a definition is to present the essence, the distinguishing characteristic of the subject we are trying to identify. A definition is to tell us what the fundamentals of a particular entity are. No statistical correlation could ever provide this. According to Salerno,

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> Measures of the U.S. money stock in current use in economic and business forecasting and in applied economics and historical research are flawed precisely because they are not based on an explicit and coherent theoretical conception of the essential nature of money. Given the all-pervasive role of money in the modern market economy, existing money-supply measures therefore tend to impede, rather than to facilitate, a clear understanding of the past or future development of actual economic events. (1987, p. 1)

To establish the definition of money we have to ascertain how the money economy came about. Money emerged because barter could not support the market economy. A butcher who wanted to exchange his meat for fruit might not have been able to find a fruit farmer who wanted his meat, while the fruit farmer who wanted to exchange his fruit for shoes might not have been able to find a shoemaker who wanted his fruit.

The distinguishing characteristic of money is that it is the general medium of exchange. It has evolved from the most marketable commodity. On this Mises wrote,

> There would be an inevitable tendency for the less marketable of the series of goods used as media of exchange to be one by one rejected until at last only a single commodity remained, which was universally employed as a medium of exchange; in a word, money. (1980, p. 45)

Similarly, Rothbard wrote that,

> Just as in nature there is a great variety of skills and resources, so there is a variety in the marketability of goods. Some goods are more widely demanded than others, some are more divisible into smaller units without loss of value, some more durable over long periods of time, some more transportable over large distances. All of these advantages make for greater marketability. It is clear that in every society, the most marketable goods will be gradually selected as the media for exchange. As they are more and more selected as media, the demand for them increases because of this use, and so they become even more *marketable*. The result is a reinforcing spiral: more marketability causes wider use as a medium which causes more marketability, etc. Eventually, one or two commodities are used as general *media*—in almost all exchanges—and these are called money. (1981, p. 3)

Since the general medium of exchange was emerged from a wide range of commodities, money must be such a commodity. According to Rothbard, "Money is not an abstract unit of account, divorceable from a concrete good; it is not a useless token only good for exchanging; it is not a 'claim on society'; it is not a guarantee of a fixed price level. It is simply a commodity" (ibid., p. 4).

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Moreover, "an object cannot be used as money unless, at the moment when its use as money begins, it already possesses an objective exchange value based on some other use" (Mises 1980, p. 131).

Why?

> In contrast to directly used consumers' or producers' goods, money must have pre-existing prices on which to ground a demand. But the only way this can happen is by beginning with a useful commodity under barter, and then adding demand for a medium to the previous demand for direct use (e.g., for ornaments, in the case of gold). (Rothbard 1981, pp. 3–4)

In short, money is that for which all other goods and services are traded. This fundamental characteristic of money must be contrasted with those of other goods. For instance, food supplies the necessary energy to human beings, while capital goods permit the expansion of infrastructure that in turn permits the production of a larger quantity of goods and services.

Contrary to mainstream thinking, the essence of money has nothing to do with financial deregulation—the essence of money will remain intact in even the most deregulated of markets.

Some commentators maintain that money's main function is to act as a means of savings. Others argue that its main role is to provide services of a unit of account and to function as a store of value. While these roles are important, they are not fundamental. The fundamental role, the essence of money, is that of a general medium of exchange. Because of this, all other functions emerge. The fact that an entity becomes the medium of exchange gives rise to these other functions.

THE DEFINITION OF MONEY

Through an ongoing selection process over thousands of years, people have settled on gold as money. In other words, gold served as the standard money. In today's monetary system, the core of the money supply is no longer gold but coins and notes issued by the government and the central bank.[1] Consequently, coins and notes constitute the standard money, known as cash, that is employed in transactions. In other words, goods and services are sold for cash.

At any point in time part of the stock of cash is stored, that is, deposited in banks. Once an individual places his money in a bank's warehouse he is in fact engaging in a claim transaction. In depositing his money, he never relinquishes his ownership. No one else is expected to make use of it. When Joe stores his

[1] It is not the intent of this article to discuss how paper money displaced gold.

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money with a bank, he continues to have an unlimited claim against it and is entitled to take charge of it at any time. Consequently, these deposits, labeled demand deposits, are part of money.

Thus, if in an economy people hold $10,000 in cash, we would say that the money supply of this economy is $10,000. But, if some individuals have stored $2,000 in demand deposits, the total money supply will remain $10,000: $8,000 cash and $2,000 in demand deposits – that is, $2,000 cash is stored in bank warehouses. Finally, if individuals deposit their entire stock of cash, the total money supply will remain $10,000, all of it in demand deposits.

This must be contrasted with a credit transaction, in which the lender of money relinquishes his claim over the money for the duration of the loan. Credit always involves a creditor's purchase of a future good in exchange for a present good. As a result, in a credit transaction, money is transferred from a lender to a borrower.

The distinction between a credit and a claim transaction serves as an important means of identifying the amount of money in an economy. Following this approach, one could easily note that, notwithstanding popular practice, money invested with money market mutual funds (MMMF) must be excluded from the money supply definition. Investment in a money market mutual fund is in fact an investment in various money-market instruments. The quantity of money is not altered as a result of this investment; only the ownership of money has temporarily changed. Including investment in MMMFs in the money definition will only lead to a double-counting thereof. If Joe invests $1,000 with an MMMF, the overall amount of money in the economy will not change as a result of this transaction. To incorporate the $1,000 invested with the MMMF into the definition of money would therefore amount to double-counting.

The fact that mutual funds offer their clients check facilities has prompted some analysts to suggest that deposits with mutual funds are similar to bank demand deposits. According to Haymond,

> If Joe sees an asset he wants to purchase, say a new car, he will not be the least concerned with whether his original money is loaned out somewhere else: he will simply open his MMMF checkbook and write the check. (2000, p.60)

However, when an individual writes a check against his account with the money market fund, he in fact instructs them to sell some of his money market certificates for cash. The buyer of these certificates parts with his money, which is then transferred to the holder of the check; money changes hands, but no new money is created.

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Furthermore, the fact that MMMF checks are employed in payments does not mean that they are money. Checks are a particular way of employing existing money in transactions.

The crux in identifying what must be included in the money supply definition is to adhere to the distinction between a claim transaction and a credit transaction. Following this principle, it is questionable whether savings deposits should be part of the money supply.

According to popular thinking, the inclusion of savings deposits into the money supply definition is justified on the grounds that money deposited in saving accounts can always be withdrawn on demand. But the same logic should also be applied to money placed with an MMMF. The nub, however, is that savings deposits do not confer an unlimited claim. The bank could always insist on a waiting period of thirty days during which the deposited money could not be withdrawn. Savings deposits should therefore be considered credit transactions with depositors relinquishing ownership for at least thirty days. This fact is not altered just because the depositor could withdraw his money on demand. When the bank accommodates this demand, it sells other assets for cash. Buyers of assets part with their cash, which in turn is transferred to the holder of the savings deposit. The same logic is applicable to fixed-term deposits like CDs, which are credit transactions.

Recently, mainstream economists have introduced a new definition of money—labeled money of zero maturity (MZM). This definition comprises all monetary instruments that have zero maturity and that are therefore redeemable at par on demand. Included in MZM are currency, demand deposits, traveler's checks, savings deposits, and deposits with MMMFs. This definition, however, suffers from the fact that it fails to distinguish between a claim and a credit transaction.

Though traveler's checks are considered an integral part of the money supply, they should not be. Traveler's checks are receipts for investment in the companies that issue them. As such, they result from a credit transaction, and therefore are not part of the money definition. Cashing a traveler's check means that AMEX or VISA will transfer money from their deposits to the holder of the check, which will not change the amount of money in the economy.

Mainstream thinking currently excludes from the money supply government deposits held in banks and the central bank. Consequently, if the government taxes people by one billion dollars, money is transferred from their deposits to the government's deposit. This is viewed just as if the money supply fell by one billion dollars. In reality, however, the money is now

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available for government expenditure, meaning that money held in government deposits should be part of the definition of money.

Incorporating all the above arguments, the money supply is defined as follows:

> Cash+demand deposits with commercial banks and thrift institutions+government deposits with banks and the central bank.

This definition shows clearly that any expansion in money supply results solely from central bank injections of cash and commercial banks' fractional reserve banking.

SHOULD DEMAND DEPOSITS BE PART OF MONEY?

Although demand deposits result from claim transactions, banks are legally permitted to regard them as the outcome of credit transactions. The legal precedent for this was set in 1811 in England with *Carr v. Carr*. The court had to decide whether the term "debts," mentioned in a will, included a cash balance in a bank deposit account. The judge, Sir William Grant, ruled that it did. Grant ruled that since the money had been paid generally into the bank and was not earmarked in a sealed bag, it had become a loan to the bank (Rothbard 1983, p. 93). So, if demand deposits are legally considered credit, how can we regard them as part of the money supply?

On this, Mises wrote,

> It is usual to reckon the acceptance of a deposit which can be drawn upon at any time by means of notes or checks as a type of credit transaction and juristically this view is, of course, justified; but economically, the case is not one of a credit transaction. If *credit* in the economic sense means the exchange of a present good or a present service against a future good or a future service, then it is hardly possible to include the transactions in question under the conception of credit. A depositor of a sum of money who acquires in exchange for it a claim convertible into money at any time which will perform exactly the same service for him as the sum it refers to, has exchanged no present good for a future good. The claim that he has acquired by his deposit is also a present good for him. The depositing of money in no way means that he has renounced immediate disposal over the utility that it commands. (1980, pp. 300–01)

Similarly, Rothbard argued,

> In this sense, a demand deposit, while legally designated as credit, is actually a present good—a warehouse claim to a present good that is similar to a bailment transaction, in which the warehouse pledges to redeem the ticket at any time on demand. (1978, p. 148)

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In short, when a depositor places his money in a savings or fixed-term deposit, he temporarily relinquishes his ownership. This, however, is not the case with demand deposits. As long as the bank does not use the money in demand deposits, it is backed one-hundred-percent by the deposited cash. Whenever banks lend part of the deposited money, they create new demand deposits that do not have any cash backing. Since the created deposits can masquerade as proper representatives of cash, they should be included as part of the money supply.

A case, could be made, however, that people who place their money in demand deposits do not mind banks using their money—which would mean that we are dealing with a credit transaction. How, then, are we to decide what money is? As long as people trade, there will always be a demand for money, which will be held either in cash or in bank deposits. Consequently, regardless of people's attitudes, once banks use deposited money, an expansion of money that is not backed by money proper is set in motion.

But one could also argue that the acceptance of checks written against demand deposits as money is conditioned upon the perceived solvency of the bank that issued them. It is true that, once the public becomes suspicious about a particular bank, bankruptcy could occur because of a "run" on the bank. This does not alter the fact that demand deposits are part of money though. All that would happen in such a case of bankruptcy is that deposits not backed by cash would disappear. Deposited cash, however, cannot disappear as long as the physical stock of money is not destroyed.

ELECTRONIC MONEY, SWEEP TRANSACTIONS, AND THEIR EFFECT ON THE DEFINITION OF MONEY

The recent introduction of electronic money seems to cast doubt on our definition of money. It would appear that deregulated financial markets create various forms of new money. Notwithstanding, various forms of electronic money, or e-money, like digital currency, are effectively claims against banks. They don't have a "life of their own." Similar to demand deposits, digital currency can function as long as individuals know that they can obtain cash on demand. According to Mises's regression theorem, the historical link between paper currency and gold is what holds the present monetary system together. Various financial innovations do not create new forms of money, but rather new ways of employing existing money in transactions (White 1996). Regardless of these financial innovations, the nature of money will never change. It will always be the thing that all other goods and services are traded for.

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Since January 1994, banks and other depository financial institutions have initiated sweep programs to lower statutory reserve requirements on demand deposits. In a sweep program, banks "sweep" funds from demand deposits into money market deposit accounts (MMDA), personal savings deposits under the Federal Reserve's Regulation D, that have a zero statutory reserve requirement ratio. By means of a sweep, banks reduce the required reserves they hold against demand deposits. As a result of the sweep program one could argue that the money definition outlined above will not cover the total money supply. This criticism, however, is misplaced, for it has nothing to do with the definition as such, but with the difficulties of measuring money, which was transferred out of demand deposits by banks without the depositors' consent. (The Federal Reserve of St. Louis provides a monthly estimate of the amount of money swept.)

CONCLUSION

As we have shown, the validity of various definitions of money cannot be ascertained by means of a statistical correlation with national income. A valid definition can be established by following the essentialist approach—that is, by focusing on the distinguishing characteristics of an entity. Contrary to mainstream thinking, we have shown that the money supply definition remains intact, notwithstanding the deregulation of financial markets and the introduction of electronic means of payments.

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MANUEL CASTELLS AND THE DECLINE OF TWENTIETH-CENTURY SOCIOLOGY

WILLIAM R. ANDERSON, JR.

THE INFORMATION AGE: ECONOMY, SOCIETY, AND CULTURE, VOLUME 1: *THE RISE OF THE NETWORK SOCIETY*; VOLUME 2: *THE POWER OF IDENTITY*; AND VOLUME 3: *END OF MILLENNIUM*. BY MANUEL CASTELLS. OXFORD, U.K.: BLACKWELL PUBLISHERS, 1998.

A quick consideration of the recent decade reveals the implosion of sociology. Several sociology departments have been merged with those from other disciplines, and as the number of undergraduate sociology majors has declined, some even have been eliminated. The discipline's research openly assumes an ideological tone palatable to only the most liberal of scholars or ignorant of laymen. Clearly, the discipline Auguste Comte once set at the apex of the sciences is in a free-fall and his grandiose vision that it be the queen of the sciences and savior of the world now mocks those hopes in its decline into the periphery of social sciences alone.

Yet, a series of books by the neo-Marxist scholar Manuel Castells has injected some new life into the discipline. It has been hailed in many circles as a rebirth of sociology in its grand classical tradition—that is, positivistic in its epistemology, anti-individualistic in its paradigm, and collectivistic in its ideology. Such giants of sociology and anthropology as Anthony Giddens and Peter Hall have referred to it as the worthy successor of the "grand theory" tradition exemplified by Karl Marx and Max Weber; that is, broad discursive social analyses that explain why society is in such a deplorable state (Fuller 1999). Anthony Orum (1999) of the University of Illinois at Chicago writes with almost religious enthusiasm that the series is "uplifting and mind-expanding, ambitious and lustrous."[1] Castells's fame is even spreading beyond academic

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[1]Response to Castells's trilogy has not been uniformly positive, however. John Boli, in America's oldest academic sociology publication, *The American Journal of Sociology*, argues

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circles as the trilogy earns features space in such mass circulation newspapers as the *Christian Science Monitor* (Slambrouck 1998, sec. 1) and the *Wall Street Journal* (Paschal 1998, sec. B)—a feat rare for sociologists of any stripe. Even the business magazine *Management Today* refers to Castells as "possibly the world's most highly regarded commentator of the information age and new economic order" (1999, p. 46).

Is all of this attention merited? Does Castells offer the scholarly as well as lay communities a new lens through which to understand contemporary society? Or, does he simply peddle the same old sociology with a techno-twist that appeals to the new suburban bourgeoisie and to an academia fatigued by the pointlessness of postmodern scholarship?

This article reviews and assesses the trilogy Castells's entitles *The Information Age: Society, Economy, and Culture*. Despite the adulation and attention the books have drawn, their ideas are strikingly similar to those sociology has proffered for the past 150 years, conceptual frameworks that more often resemble ideological interpretations than a scientific praxeology. What passes for scientific sociological theory and analysis actually proves to be little more than an expression of the sociologist's own preferred vision of the world—in Castells's case, a "social democratic" one (Lloyd 1998, pp. 13–14). As Mises points out in *Theory and History* (1985, p. 250), "Collectivism transforms the epistemological doctrine [of conceptual realism] into an ethical claim." If such is the case, then Castells's work is refreshing and stimulating because of a collectivist ethos made enticing by its hip, techno theme, not its pathbreaking sociology. If so, then the *information* age closely resembles a futuristic myth instead of science, yet another confirmation of sociology's demise, not its resurrection.

Indeed, much of *The Information Age* reminds one of what sociology has fallen to in the last half-century: (1) political bias hidden by the so-called objective scientific method, (2) exaggerated claims for the conclusions of its research, and (3) an ongoing social vision that seeks to reconstruct society in a way that emphasizes collective over the individual, social uniformity over liberty.

But, first, some background about Dr. Castells. Born in Barcelona, Spain, in 1942, he fled Franco's regime for Paris during the 1960s. Like many of his contemporaries in sociology, Castells's formative experiences took place during that era. As a professor at the University of Paris, he saw and participated firsthand in the 1968 student uprisings—a fact he often cites in the trilogy. In 1979, the University of California at Berkeley appointed him professor of

that Castells's interpretations are too "neat and tidy," and lack the "institutional and historical incisiveness so characteristic of Marx and Weber." See also Boli (1999, pp. 1843–44).

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sociology, and he later assumed chairmanship of the Center for Western European Studies there. In the ensuing years, Castells's prominence has only increased. In 1995, he was appointed to the European Commission's High Level Expert Group on the Information Society. Recognizing his stature in the field of sociology, the American Sociological Association in 1998 awarded him one of its highest honors, the Robert and Helen Lynd Award for distinguished career contributions to community and urban sociology. Though he labels himself a European "social democrat" worried about losing the political gains made in Europe, he admits that, when it comes to the Marxism he claims to have forsaken, he still employs it as a "tool" (Lloyd 1998, p. 13).

Much of Castells's earlier work was in urban sociology (specifically grassroots urban political movements) and included books such as *The Urban Question* (1979) and *The Informational City* (1989). These volumes lay much of the groundwork for what would follow in *The Information Age*.

Not until the early 1990s did Castells's thinking hit its pace. For years, he had been studying American and European cities. What he discovered, he believed, is a new form of social organization. No longer does the industrial-capitalist configuration of society hold. Instead, information is becoming the new capital, information networks the new means of production and influence organizing contemporary social life. It is this theme that *The Information Age* develops.

THE TECHNO-MARXIST METHOD: THE INFORMATION AGE TRILOGY

From his opening volume, Castells's method reveals his reliance upon Marxist presuppositions and categories. He employs first a dialectical and historical materialism and then the base and superstructure distinction so characteristic of Marxist argument. The analytical categories (class, ideology, etc.) may be different, but the conceptual framework is strikingly similar – historical conditions creating a structural determinism that, in turn spawns the struggle and conflict that evolves a new synthesis of forms. "What truly matters for social processes and forms making the living flesh of societies," Castells writes,

> Is the actual interaction between modes of production and modes of development, enacted and fought for by social actors, in unpredictable ways, within the constraining framework of past history and current conditions of technological and economic development. (vol. 1, p. 18)[2]

[2]Another representative quotation comes from the opening of the trilogy's conclusion.

> A new society emerges when and if a structural transformation can be observed in the relationships of production, in the relationships of power,

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The series is divided into three books, and almost dialectically. Volume 1, subtitled "The Rise of the Network Society," presents Castells's findings on the new material conditions of capitalism. Information networks, not industry or capital versus labor, are the heart of a new social morphology. To be included in these information networks is to have access to power and control. It is this network structure, not human action, that increasingly defines the Informational Society.

Networks take many different shapes. They may be combinations of political elites who manage the European Union, international markets, global criminal organizations, even television systems. Common to all is their internal flow of information, which turns information networks into social networks. This gives groups, regardless of their composition, a dynamism that responds to needs and influences from other networks. Often, information and social networks overlap and bring about new configurations of power. In these cases, influence and power then rests in the "switches" that connect people and groups—that is, gatekeepers of information and capital (vol. 1, p. 471).

Behind our new society lies the technological revolution that has turned information into the new vehicle for capital. Technology rejuvenates capitalism by injecting speed and connecting people, parties, and corporations in new ways. New technologies not only overcome political systems but transcend cultural differences among nations. Computers and the Internet transform global diversity into unity and, eventually, homogeneity. "The new economy is organized around global networks of capital, management, and information, whose access to technological know-how is at the roots of productivity and competitiveness," concludes Castells (p. 471). These networks cut across geographic, national, corporate, and even demographic boundaries.

We have, consequently, a new social order tuned to technological educability and a new stratification system calibrated upon varying educational levels. The educational institution is central, in other words, and reorganizes the great cleavages in the Network Society on new terms. Education equips information producers to acquire new knowledge, empowering them to generate new information that then fuels capitalist expansion. Those without education generally cannot absorb and create new information, which leaves them as dregs of the Information Society, to be exploited as expendable labor and, hence, marginalized.

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and in the relationships of experience. These transformations lead to an equally substantial modification of social forms of space and time, and to the emergence of a new culture. (vol. 3, p. 340).

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Volume 2 advances Castells's argument beyond social structure and capitalism, and moves into the antitheses they trigger, cultural reactions and alienation. Indeed, the new "class struggle" is not between capital and labor but between the informationally networked and the marginalized. The networked are the "included" (my term) and powerful; they have access to the networks underlying the Informational Society. The marginalized, on the other hand, are the "excluded," one might say—those used, then cast aside with the confidence that they bear no threat to the new order.

Though peripheral to the Network Society, the excluded are not going away. Instead, they resist. The new information order tries to bleach their identities and experiences to rid them of any remaining individuality. The excluded, then, become more aggressive, more assertive of what positions them in counter-distinction to the new age—their identities and personal experience, both of which are deemed irrelevant by the networked. This is why the volume is subtitled "The Power of Identity."

Here, some of Castells's observations are striking, indeed constitute the strongest portion of the trilogy. The cultural hegemony of the Informational Society is far from complete, he reminds the reader. Across the globe, social and political movements have emerged to combat the monolithic cosmopolitanism of the new world order and its increasing reach into the fabric of our daily lives. These cultural resistance movements fight to reinfuse their existential meaning into their own social worlds as well as the Network Society. Ironically, to do this, the excluded must mimic the networking and cultural codes of the dominant order, but with two exceptions: first, their code is near-tribal and individualistic instead of cosmopolitan, and, second, their networking is decentralized, from the standpoint of being outside the dominant networks and political alliances of the Information Society, even though unified in their opposition to the powerful.

The crosscutting power of the Information Society thus erodes the industrially-based political and cultural movements such as labor and political parties. New identity movements arise. They make use of the same Information Society technologies to create communities of opposition and call into question the legitimacy of the State itself. Hence, the nation-state faces difficult times in the Information Society. It is undermined from outside (networked global capitalism) and from within (internal networks spreading horizontally across a nation and the globe).

Castells identifies several varieties of identity movements. Cultural communes are defensive identity movements that typically offer a communal haven based upon common religious values, collective memory, or even territory.

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They include religious fundamentalisms (Christianity, Judaism, Islam, etc.) and cultural nationalisms that retreat into a closed society in order to cope (chap. 1).

A second type of identity movement specifically organizes against the new global order (chap. 2). Among these, Castells includes Mexico's *Zapatistas,* who effectively—even brilliantly—utilized media information networks and the Internet to organize a global network of solidarity in their struggle against the Mexican government during the early 1990s. The American patriot and militia movements and the Japanese apocalyptic movement *Aum Shinryko* are other examples. Both of these latter movements effectively employ telecommunications, particularly the Internet, to organize networks of resistance.

Last, what Castells calls "project identities," factions such as environmentalists and feminists, work to reconstruct society in their own images (chaps. 3 and 4). These social movements occupy the new arena of cultural struggle, their power deriving not from territorial or class-based strength but from their ability to utilize their own and other' informational networks to shape and re-code even the networks of the "Included."

Volume 3, subtitled "End of Millennium," incorporates global transformations and futuristic speculations into Castells's theories—a synthesis of trends, one might say, set into motion by the dialectic between the globalization of wealth and information and the localization of identity and legitimacy.

Castells begins this final volume with a review of the collapse of Soviet-style communism and, from there, moves to the transformation of Chinese-style communism to global capitalism (chap. 1). Here, as elsewhere, his ideology fails him. With both, he ignores the inherent contradictions of command economies as pointed out by Mises (1981, pp. 97–105)—that is, the insurmountable challenge of having no basis for "economic calculation." Instead, he attributes their demise and change to internal contradictions of statist societies or the perversions of their governing ideology, not the logic of action underlying their economies.

He also reviews several other regions of the world, some of which he labels "Black Holes of Informational Capitalism" (chaps. 2–4). These are pockets of social exclusion from which there is no escape in the Informational Society. Poverty-stricken regions of central Africa and urban ghettos are examples of these. Other regions fitting into the "Black Hole" category include the Pacific Rim and the new European Union. He even explores the sociocultural impact of a rising global criminal class.

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By the conclusion of his trilogy, Castells's broad reach has touched topics ranging from media to international politics to liberation movements to criminality. He predicts that the revolution in informational technology has attained an unstoppable momentum. Information-getting will continue its path toward decentralization through the Internet and such mobile technologies as cell phones, which will expand the reach of capitalism into all corners of the globe. Governing the economy will be multilateral organizations comprising their own network of influence and power. G-7 and the International Monetary Fund are two examples. The United Nations and NATO will be deployed as their policemen against increasingly irrelevant but still threatening nation-states. Those people excluded by these changes will opt for criminality or identity resistance movements as their lives become more out of touch with the logic and movement of the information age.

THE DEATH OF SOCIOLOGY?

Permit me to preface my answer to this question by noting that Castells's work is impressive, to say the least. It is dazzling in its comprehensiveness, aggressive in its rethinking the nature of contemporary society, and at times breathtaking in its breadth. His insights, especially those about cultural resistance in volume 2, are original and insightful. In so many respects, he captures the dialectical tension between self-ordered liberty and the tyranny that marks our statist age.

And yet, the trilogy also reflects the same political bias and epistemological confusion that plagues so much of sociology. The result is almost a kind of myth that purports to ask and answer the pressing questions of the information age, but in actuality cannot.

Political bias, for instance, seeps inevitably into Castells's classifications and analyses, especially when he discusses social movements in the Network Society. He denies them any role other than reactionary. They are what Mises (1985, p. 254) labels "refractory rebels." Environmentalism, feminism, and other so-called "progressive" movements are labeled "project movements" that seek reform. But fundamentalisms (religious, political, etc.), no matter how proactive, are labeled defensive and resistant. One might just as easily reverse the labels. In that case, the configuration of social movements in the Network Society would be quite different. Fundamentalisms would then be interpreted as fighters for diversity and human liberty against the monolithic global society, with environmentalism and feminism as revolutionary movements trying to impose collectivist agendas that undermine a prosperous and free society. In other words, Castells too easily dismisses conservative movements as being regressive and too easily accepts liberal movements as

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progressive. This bias colors how he conceptualizes the dynamics of the Network Society.[3]

The Information Age also reveals a misleading use of sources. Sometimes, as Steve Fuller (1999, p. 162) of the University of Durham points out, Castells marshals aggregate data without commenting on the shortcomings of comparing summary statistics across nations. In other cases, he accepts the reports of such highly politicized organizations as the Southern Poverty Law Center and treats their statistics as valid without explaining the center's criteria of classification. This misleads the uncritical reader into accepting methodological artifacts as statistical fact supporting what is a questionable interpretation.

And, third, the books offer examples of poor argumentation—usually when the aggregate statistics fail to support Castells's interpretation. For instance, in volume 2, Castells describes the rise of militia movements and attempts to show (based upon unquestioned Southern Poverty Law Center data) their spread across America. When even those figures do not support his argument, he then throws in the following: "If we consider the Christian Coalition to be a part of the [militia] movement, then Patriots are present in the suburbs of most large metropolitan areas" (vol. 2, p. 95). Castells creates—out of thin air and without scholarly proof—a presumed, if not desired similarity and treats it as accepted fact. This sleight of hand allows him to magically pull rabbits out of a hat that isn't even a hat.

These problems expose the fundamental weakness of sociology's claim to being a science, for they reveal the political undercurrents of so-called scientific sociology and its inability to remain "value-free." For example, Castells writes that, "[The Informational Society] is "based on the historical tension between the material power of abstract information processing and society's search for meaningful cultural identity" (as if society can "search" for anything; it is individuals who act, not collectivities). "Statism," he concludes, "seems to be unable to grasp the new history" (vol. 3, p. 67). He then goes on to add that information technology gives networks of people the power to circumvent statist policies and structures. This new form of politics will render the state irrelevant, if not obsolete.

So far, so good. Castells cannot bring himself to stop there, however. His Marxist "tool" fails him, just as Mises predicted it would. Marxism, like collectivist sociologies, inevitably leads to polylogism (Mises 1998, p. 75; Gordon

[3]Castells violates one of Mises's chief maxims. In *Human Action*, Mises comments that "Those writers who consider historical events as an arsenal of weapons for the conduct of their party feuds are not historians but propagandists and apologists. . . . [They must] be neutral with regard to any value judgments" (1998, p. 48; see also, pp. 10 and 879–81).

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1993, pp. 123–25). Castells's brand is no exception. He assumes that people have no individual minds. They merely think and reflect what the group thinks. The included think one way, and the excluded another.

Castells's polylogism thus blinds him to the fact that individuals engage in purposeful action, and that the logic behind such action will be similar regardless of their respective social positions—not just those individuals in identity movements. Like the identity movements, the guardians of the Informational Society and the State will reassert their identities and control, since their action, too, is purposeful. He does not consider the ever-present likelihood that the State will not readily accept its delegitimacy at the hands of the networked society, especially from those at its margins. It may either attempt to recentralize informational networks by exerting power through regulation, or merge with other states to create a global mega-State fully overwhelming the Informational Society and quashing, as revolutionary threats, identity movements of all stripes.

Castells manifests the collectivist's desire that history and materialism have an inevitable momentum that predetermines individual action and choice. It is these impersonal forces, not individuals, they would have us to believe, that act on the world stage. Had Castells taken human action as his starting point, his analysis would not have fallen into these traps. It is, after all, individuals engaging in purposeful action who comprise society. Castells would have understood the information age differently—and truly. The New World Order then would be seen for what it is: a complex phenomenon resulting from the interplay of human choices toward desired ends.

Sociology, to be a comprehensive human science, must focus upon individuals and their actions, not the social structures and collectivities that supposedly produce them. To do otherwise is to remove humans from the picture and erroneously emphasize the group over the individual. In other words, to discount individual action is to lapse into a one-sided determinative collectivism. As Rothbard notes,

> The true science of man concentrates on the individual as of central, epistemological and ethical importance; the adherents of scientism, in contrast, lose no opportunity to denigrate the individual and submerge him in the importance of the collective. With such radically contrasting epistemologies, it is hardly sheer coincidence that the political views of the two opposing camps tend to be individualist and collectivist, respectively. (1997b, pp. 22–23)

Indeed, we are reminded of sociology's exhaustion when we look at Castells's methodological starting point. There is nothing new here. Like earlier

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sociologists, he treats society and events as if they can be examined with the same lens as a natural scientist would employ. But as Rothbard, again, points out, the natural and social sciences must be distinctive in their methodologies for a basic reason: The properties of their subject matters are different (1997c, p. 34). Human behavior is not mechanistic because "humans engage in motivated, purposeful action," Rothbard adds. Hence, any thought that social science can produce exact laws similar to those of the physical sciences is illusory, even dangerous. And yet, the holistic and deterministic dialectic employed by Castells suggests rigid laws of social physics wherein inevitability, not possibility describes the path of history and the future. Castells cannot help it. Once he tosses out individuals and replaces them with collectivities, sociological fatalism is set into motion. Unfortunately, there is nothing new here either; it is simply the same old flavor of sociology but with a cyber-candy coating.

In *Human Action,* Mises explains the inevitable trap into which these holistic arguments fall. They do not begin, as they should, with the self-evident axiom that "action is always the action of individual men" (Mises 1998, p. 143). Rather, holistic theories start with society or institutions. Because they presume collectivities as primary instead of derivative of individual action, they force the individual to reconcile with and integrate into society, thus stripping away the concrete and practical truth that individuals, not collectivities, act. With that accomplished, deterministic and linear theories are easy to construct, for they subdue the individual's ends—despite being a free and active being—to society's. By absolutizing both material and collective spheres, Castells slips into applying a universal method of interpretation for what is only a narrow, particularistic portion of social life. When individuals are treated only as by-products of collective influences, that is easy to do. It removes the inconvenience that real people in the real world (not some theoretical construction) actually think and act because of a variety of motives. It is then but a short step to telling people what they ought to do.

This is why such systems are not "science" at all. As Mises notes a few pages later, these holistic theories require a move from science to metaphysics and theology (ibid., p. 145). So we should not be surprised that sociology, when it eliminates human action from its interpretation, becomes an exercise in social and political metaphysics masked as "scientific" analysis.

When that happens, sociology becomes myth, not science. It fits the events of the social world into a preconceived interpretive framework that imposes meaning upon them. Hence, using *The Information Age* as an example, there are all-powerful, arbitrary gods (the Informational Society) who rule our lives,

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moving us about as if we were pawns in their vast chess game. There are good spirits (environmental and feminist movements) versus bad spirits (those controlling and uniting through the information networks). There are the misguided spirits, deluded by their own sinfulness (fundamentalisms and patriotic movements). There even is a soteriological element in the model: as Castells writes, "the most fundamental political liberation is for people to free themselves from uncritical adherence to theoretical or ideological schemes, to construct their practice on the basis of their experience" (vol. 3, p. 359). Alas, if only Castells had followed his own advice.

At the turn of the last century, sociology held great promise. It had its perversions, certainly (the excesses of Durkheim come to mind). But two of its intellectual giants, Max Weber and Alfred Schutz, actually proposed the nomothetic agenda prescribed by Rothbard. Both Weber and Schutz believed that no science of society is possible without beginning with the conscious choices of individuals resulting in purposeful human action.[4] Unfortunately, they have been in the minority as the discipline has increasingly been captured by two sides of the same epistemologically erroneous coin—the positivists and the Marxists—who have pushed sociology to near irrelevancy through their obsession with quantification or collectivization. Is it any wonder, then, that sociology is dying? It cannot even focus its attention on what it should study—human action.

Sociology has been reduced to propagating a myth. Mythology is not the path of life, however, it is static, if not regressive. It condemns men to ignorance, inaction, and passivity before the relentless oppression of its worldview. Mythology brings, in other words, decay and death, for it suffocates the dynamism that advances societies, that is, the creative and entrepreneurial impulse within every person. "Society," Mises reminds us, "lives and acts only in individuals; it is nothing more than a certain attitude [of cooperation] on their part." He concludes; neither the gods nor a mystical "Natural Force"

[4]It is no coincidence, then, that Mises comments favorably on both Weber and Schutz. Both were moving toward a science of human action that considered methodological individualism essential to its epistemology, and an actor's process of valuation key to understanding his action.

Schutz regularly joined in Mises's famous private seminar in Vienna (Mises 1978, p. 100).

Mises and Weber became good friends during the last year of World War I, when Weber lectured at the University of Vienna. Mises considered the early death of that "genius" a blow to Germany (ibid., pp. 70 and 104). In Mises's judgment, Weber probably would have employed the term "praxeology" had he known of it (Mises 1998, p. 126) – yet not without problems. His error comes in distinguishing rationally based praxeology from other types of praxeology. All human behavior is praxeological; what differs is the valuation toward particular ends. Mises also was critical of Weber's use of "ideal types" (Mises 1978, p. 123).

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created society, it was created by mankind. Whether society shall continue to evolve or whether it shall decay lies – in the sense in which the causal determination of all events permits us to speak of free will – in the hand of man (Mises 1981, pp. 468–69).

And therein rests the lifeblood of any true sociology – the study of human action.[5]

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[5]Though beyond the scope of this review, an intriguing question is: why hasn't economics suffered the same fate as sociology in the twentieth century? It, too, welcomed Marxian thought and positivism, propagated myths, and succumbed to political ideology.

A hint toward an answer may rest in the fact that economics has its roots in Aristotelian realism, thus giving it a stronger mooring to the real world of human action. During its formative years, economics had a clearly articulated alternative, emphasizing praxeology, upon which subsequent scholars of "realist" convictions could draw upon to save the discipline from itself, so to speak. Rothbard points out that, in the late 1600s, Sir Dudley North's methodology for analyzing trade incorporated a praxeological method later to be picked up by Richard Cantillon, Jean-Baptiste Say, Nassau Senior, and eventually Ludwig von Mises (see, further, Rothbard [1995, p. 324]). Thus, economics has not imploded like sociology thanks to its proffering of a paradigm quick to refuse the fallacies of polylogism, holism, and positivism.

Sociology, in contrast, has not been so fortunate. Its philosophical foundation rests upon the French Enlightenment optimism of Rousseau, and its intellectual origins extend to the philosophies of the madman Auguste Comte and the revolutionary Karl Marx, neither particularly committed to methodological individualism (except when it concerned their own "genius"). Subsequent to them came the methodological holist Emile Durkheim, as anti-individualistic in his method as is possible. Hence, sociology's lack of a grounding in realism has made it susceptible to further holistic variations and, in a sense, myth-spinning.

Because of sociology's holistic, antirealist premises, new theories remain holistic despite changes in concepts or emphases, leaving few avenues for an alternative methodology to emerge and develop. Indeed, even this review illustrates the problem. To critique Castells's sociology requires leaving the discipline for the fully articulated praxeology found in Austrianism. Other recent examples may be found in the excellent work of Kenneth H. Mackintosh (1999a, pp. 67–77; 1998–1999, pp. 103–23) and his review of Infantino's *Individualism in Modern Thought: From Adam Smith to Hayek* (1999b, pp. 87–92). All of this suggests the rich potential for a sociology reformulated on praxeological principles.

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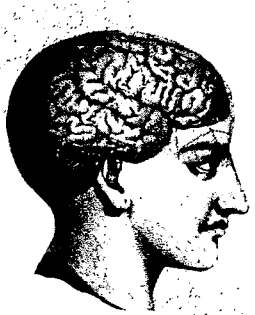

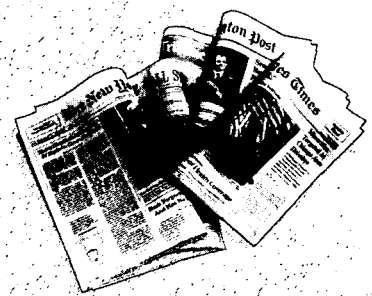

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IN RESTRAINT OF TRADE: THE BUSINESS CAMPAIGN AGAINST COMPETITION, 1918–1938. BY BUTLER SHAFFER. LEWISBERG, PENN.: BUCKNELL UNIVERSITY PRESS, 1997.

Ask a random sample of educated Americans why government regulation of economic affairs has expanded over the past century or so and the modal response will be something along the following lines: in the late nineteenth century, the Robber Barons created Big Business. From that time onward, it became ever clearer that capitalist "excesses" and "abuses" were inherent in an unfettered market economy. Therefore, "the people" demanded that their political representatives impose limitations on and government regulation of the actions of business firms in order to protect the public from those excesses and abuses. Hence the succession of measures that stretch from the Interstate Commerce Act and the Sherman Antitrust Act, in 1887 and 1890, respectively, to the Securities Exchange Act, the National Labor Relations Act, and a host of similar measures created during the 1930s, and on to the multitude of other regulatory laws enacted right up to our own day. One gets an image of raging free enterprisers on the one hand and public-spirited reformers on the other. This understanding, however, is for the most part a myth.

Consider the following statements by prominent actors on the political stage.

> The laissez-faire economy which worked admirably in earlier and simpler industrial life must be replaced by a philosophy of planned national economy.
>
> We must put aside individualism; we must put aside unenlightened selfishness and stand together as one in a great irresistible push with stability and prosperity for all as our goal.

The words of starry eyed radicals or ivory tower intellectuals? Hardly. The first quotation belongs to Henry I. Harriman, president of the U.S. Chamber of Commerce, the second to B.B. Gossett, president of the American Cotton Manufacturers Association. Each (quoted by Shaffer, pp. 109, 203) dates from 1933, the very dawn of the New Deal's effort to—as popular wisdom would have it—"tame" rampaging capitalists.

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Butler Shaffer's well-written monograph, *In Restraint of Trade*, describes in extensive detail why and how most businessmen pleaded for the government to tame them between the end of World War I and the eve of World War II. Other scholars have plowed this field before: most notably John T. Flynn (to whose memory and spirit Shaffer dedicates his book), writing from the 1920s to the 1950s; Gabriel Kolko, Robert H. Wiebe, and Murray N. Rothbard, writing in the 1960s; and many others since. Shaffer's contribution is simultaneously to ground his analysis in sound economic theory and to document his historical claims by citing an abundance of primary sources. He goes into special detail in three chapters devoted to particular industries or sectors of the economy—steel (chap. 5), petroleum and coal (chap. 6), and retailing and textiles (chap. 7)—but the entire book is scrupulously documented.

The problem businessmen faced, of course, arose from their inability to suppress competition among themselves. When they met, "even for merriment and diversion," their conversations tended to end pretty much as Adam Smith indicated: "in a conspiracy against the public, or in some contrivance to raise prices." But the conspiracies wouldn't hold up: typically sooner rather than later, individual firms broke the price-fixing or supply restraining rules in order to increase their own profit at the expense of their fellow conspirators. Lacking the power to coerce one another into compliance, firms couldn't create effective, durable cartels.

Smith, one may recall, added to his famous observation about the conspiring businessmen the admonition that, although the law should not hinder people of the same trade from meeting, "it ought to do nothing to facilitate such assemblies; much less to render them necessary." But what Smith advised against was precisely what businessmen succeeded in getting the government to do from World War I to the late 1930s. First, "with the trade associations helping to supply the coordination, the WIB [War Industries Board] politicized the bulk of the economic life of this country during World War I" (p. 21). As Bernard Baruch, head of the WIB, observed, "hundreds of trades were organized for the first time into national associations . . . on the suggestion and under the supervision of the Government" (quoted by Shaffer, p. 25). Then, during the 1920s, goaded by Secretary of Commerce Herbert C. Hoover, trade associations lined up businessmen to conform to "codes of ethics," the main objective of which was to rein in "the chiseling minorities" who would not "play the game" (quoted by Shaffer, p. 57). Shaffer devotes an entire chapter to discussing those codes. Again, of course, businessmen found that, lacking coercive power, they could not keep these minorities from chiseling. Also in the 1920s, the Federal Trade

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Commission organized a number of "trade practice conferences" that proved yet another futile attempt at suppressing "destructive competition." With the onset of the depression, "business leaders became more militant in their proposals for stabilizing trade conditions" (p. 98). The Swope Plan, brainchild of General Electric president Gerard Swope, and similar proposals were put forward in the early 1930s, eliciting favorable responses from most businessmen.

By that time, businessmen were ready to sell their souls for relief from the rivalry. According to a May 1933 editorial in Business Week, "The American business man at this moment is utterly weary of the ruthless competitive struggle. It has been too much for him: he has survived so far, but he is spent. He is willing, he feels just now, to surrender some part of his freedom of action to achieve a degree of stability" (quoted by Shaffer, p. 109). Surrender he did, falling into the loving arms of the National Recovery Administration, the federal enforcer of the more than 700 cartels created under the authority of the National Industrial Recovery Act of 1933 (NIRA). So pleased were the nearly spent competitors that one of them judged the NIRA "almost worth the price of the depression" (ibid.). General Hugh Johnson, the Baruch crony tapped to head the scheme, praised it even more lavishly, calling it a "Holy Thing . . . the Greatest Social Advance Since the Days of Jesus Christ" (quoted by Shaffer, p. 106).

Although the NIRA industrial codes differed in various ways, their common denominator was an attempt to restrict market supply, suppress competition, and thereby prop up prices. Although the grand scheme never achieved universal compliance, it did have some effects, one of which surely was to slow the economic recovery that had begun in early 1933. Shaffer takes issue with the oft-stated view that few tears were shed when the Supreme Court invalidated the NIRA in early 1935. In his judgment, "The commonly accepted notion that American business breathed a collective sigh of relief over the demise of the NRA is inaccurate. The business opposition that did develop tended to focus more upon *administrative* difficulties than upon the principle of government enforcement of industry-determined trade standards" (p. 117). In addition to the direct evidence Shaffer presents of political support for continuation of the NIRA, his thesis also receives support from the fact that, in particular sectors or industries, NRA-style arrangements were quickly created after the "Mother of All Cartels" was tossed out. Petroleum, coal, truck transportation, airline passenger transportation, agricultural marketing, food and drugs, retail, and labor services (via the Wagner Act) all provide examples.

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In his broad, interpretive discussions, Shaffer sees the growth of government and of mercantilist privileges for business interests as symbiotic. Contrary to conventional wisdom, "government intervention has been invoked *not* at the behest of persons who saw the market failing to function properly, but at the prompting of business interests who were concerned that the market was functioning all too well" (pp. 212–13). Shaffer regards business pressure as the prime mover in the growth of government intervention in the economy. He does not establish that conclusion, however, for he looks only at business actions and does not attempt to compare or contrast them with the actions taken by others who sought enlarged government. Nor does he consider factors—the development of a congenial dominant ideology or the occurrence of a great national emergency, for instance—that might have provided conditions necessary for the success of the business lobbyists. Without a doubt, however, he demonstrates that business interests were in the thick of the push for government intervention.

The book has a dated feel. Only a few items in the long list of references were published after 1990. Although I doubt that anything in the substantial related literature produced by economic historians during the past decade would invalidate Shaffer's work in any important respect, citation of that literature would have helped to secure his book a place on the short shelf of essential monographs on this topic, a place it well deserves.

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WHEN IN THE COURSE OF HUMAN EVENTS: ARGUING THE CASE FOR SOUTHERN SECESSION. BY CHARLES ADAMS. LANHAM, MARYLAND: ROWMAN AND LITTLEFIELD, 2000.

Earlier this year, the *Wall Street Journal* reported the breaking of new scientific ground in the study of civil wars. Economists at Oxford had proven beyond a statistical doubt that civil wars have economic causes and are fought for economic reasons. Ludwig von Mises made this all-important insight more than fifty years ago.

In the current ideological war, American intellectuals and their minions in the media and arts try to paint the founding fathers, especially Thomas Jefferson and George Washington, as racist slave owners out to establish a new American hegemony based on black slavery. The whole republican period is portrayed as a pure free-market economy that wrought unprecedented capitalistic exploitation, especially of minorities and labor. From this ideological vantage point, the war between Union and Confederate governments becomes the true battle for freedom, with democracy in the vanguard, the slave system vanquished, and Lincoln established as the messiah.

Most Americans are taught that slave owners started the war to protect their power and position while the people of free states to the north rose up to emancipate their oppressed black brothers and sisters. The economic view of war contradicts the idea that the South and North fought to preserve or destroy slavery. The true catalyst of this or any other war is not racial, religious, or tribal, it holds, but economic.

The great value of Charles Adams's book, *When in the Course of Human Events: Arguing the Case for Southern Secession*, is that it shows in careful historical detail that slavery did not cause this great tragedy. In short, the Southern states formed a new republic to avoid paying tariffs that benefitted northern industry and paid for the construction and operations of federal government facilities and other "public works," most of which were located in Northern states.

With secession, trade and tariff revenues were diverted to the South. This led the "money interests" in the northeast to force Lincoln to start a war with the Confederacy in much the same way Roosevelt dragged America into

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World War II. We now know conclusively that Roosevelt knew the Japanese would attack Pearl Harbor, and that he deliberately incited and provoked them with acts of war (Stinnett 1999). Adams builds a sound factual case that finds Lincoln deliberately starting war with the Confederate states in a similar manner.

Among the hundreds of books on the American Civil War, Adams provides the most accurate and detailed account of what caused that war. His logical analysis provides for an insightful perspective on the consequences of the war that led to one of the worst slave emancipations in history, as well as to the horrors of Reconstruction, which Adams rightly considers the fount of many of our current race-related problems. His retelling of the "trial of the century" should crack even the most ardent Union apologist into a more honest viewpoint of these *human events.* Adams is an impartial historian who writes with a clear and engaging style. This is a great book that everybody should read.

One criticism, however, is that Adams considers the war to have been a hopeless venture for the Confederacy because the North had vastly superior armies. The economic approach to war emphasizes how each nation deploys its resources rather than the absolute size of its armies or resources. The author's view is therefore curious, especially given that the Confederacy came so close to winning its independence, and is even more puzzling given the author's own conclusion that the South beat the North's massive army of occupation during Reconstruction with an ill-equipped and unorganized guerrilla army whose operations were carried out under the guise of the Ku Klux Klan. This, however, is a minor criticism of a point unrelated to the main topic and contributions of an otherwise outstanding book.

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